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PROCEEDINGS

OF

THE ROYAL SOCIETY.

January 6, 1887.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Occurrence of Silver in Volcanic Ash from the Eruption of Cotopaxi of July 22nd and 23rd, 1885." By J. W. Mallet, M.D., F.R.S., University of Virginia. Received November 26, 1886.

A few months ago I received from Señor Julian R. Santos, of Ecuador, formerly a pupil of mine in the laboratory of this University, a specimen of volcanic ash collected at his place of residence, Bahia de Caraguez, on the coast of the Pacific, about 120 miles nearly due west from Cotopaxi. This, the highest and among the most mighty of the active volcanoes of our globe, burst forth into eruption about 11½ P.M. on the 22nd of July, 1885, and the ash began to fall at Bahia de Caragues at 7 A.M. on the next day, the 23rd. It fell there to the depth of several inches, this fact alone indicating the discharge of an enormous amount of solid matter into the atmosphere, although Señor Santos wrote to me that the unsettled condition of the country, disturbed by revolutionary movements, prevented his making extended enquiries which might have ascertained the area covered by the fall of ashes.

The specimen sent me consisted of a very finely divided powder, mobile and soft to the touch, of light brownish-grey colour. Under the microscope it appeared to be made up of minute granules and spicules, in general with sharp, more or less splintery edges. These were for the most part colourless and transparent, or white and translucent; some were reddish, some dark bottle-green, some brown, some black and opaque. Most of those clear enough to freely transmit light showed brilliant colours in a field of polarised light. Quartz, two felspars (one white, and one pink or reddish), augite, magnetite (strongly attracted, and easily removed by the end of a magnetic needle), and thin scales of deep red specular iron oxide were easily distinguished

The ash on being strongly heated before the blowpipe, or even in considerable quantity in a small platinum crucible over the blast lamp, turned dark red brown and fused to a nearly black slag

On being boiled in its original state with water it gave up 0 21 per cent of soluble matter The solution gave very distinctly the reactions of chlorine, a sulphate and sodium, in a less marked degree the reactions of potassium On boiling with strong hydrochloric acid, 6 94 per cent was dissolved in addition to that already extracted by water, the acid solution was deeply coloured by iron

The specific gravity of the ash was found = 2 624 at 18° C as compared with water at the same temperature

An analysis of the material taken as a whole i e without any previous mechanical separation of its consistent minerals, and without previous digestion with water or acid but dried at 100° C, gave the following results —

SiO_2	56 89
TiO_2	trace
Al_2O_3	19 72
Fe_2O_3	4 06
FeO	3 65
MnO	trace
MgO	1 91
CaO	5 87
Na_2O	5 14
K_2O	1 96
Li_2O	trace
Ag	,
Cl	,
SO_4	,
PO_4	
H_2O	0 62
	99 82

Silver was first noticed after fusing as usual with mixed sodium and potassium carbonates, and dissolving in excess of hydrochloric acid, on the addition of sulphuretted hydrogen to the solution, which had been freed from silica, the sulphur thrown down by ferric chloride present was observed to be distinctly brown and on being filtered out and carefully burned off before the blowpipe it left a minute bead of metallic silver All the reagents and vessels used were scrupulously examined, but the silver could not be traced to any of them It was afterwards found that the metal could be obtained from the ash by furnace assay—fusion with pure lead carbonate, sodium carbonate, and a little cream of tartar, and cupellation of the lead button produced; and a comparative experiment was made, with negative result, using larger quantities of the same reagents, but omitting the volcanic ash.

It was ascertained that silver could be extracted from the ash by boiling it with a solution of ammonia, or of potassium cyanide, or of sodium thiosulphate, but the metal was not dissolved out in appreciable amount on boiling with nitric acid. Hence, as seems most probable, it was present in the ash as silver chloride. The fact of its being found in the solution in hydrochloric acid of the mass resulting from fusion with the alkaline carbonates, is of course easily explained by the solvent action upon silver chloride of the chlorides of sodium and potassium, and (when such minute quantities are concerned) of hydrochloric acid itself.

The discovery of silver in the ash in question adds for the first time this metal to the list of elementary substances observed in the materials ejected from volcanoes, and the addition derives some special interest from the fact of the ash having come from the greatest of the volcanic vents of the great argentiferous chain of the Andes.

Lead, which was found by Señor Santos himself, when a student here in 1879, in a specimen of ash from the eruption of Cotopaxi of August 23rd, 1878,* was sought for in the ash now reported upon, but neither it nor any other heavy metal beside silver was detectable.

Several concordant experiments proved that the silver was present to the extent of about 1 part in 83,600 of the ash, or about two-fifths of a Troy ounce per ton of 2240 pounds. Small as is this proportion, it must represent a very large quantity of silver ejected during the eruption, in view of the vast masses of volcanic ash which must have been spread over such an area as is indicated by the fall at so distant a point as Bahia de Caraguez.

II. "Preliminary Note on the Continuity of the Liquid and Gaseous States of Matter." By WILLIAM RAMSAY, Ph.D., and SYDNEY YOUNG, D.Sc. Communicated by Prof. G. G. STOKES, D.C.L., P.R.S. Received November 30, 1886.

For several years past we have been engaged in an examination of the behaviour of liquids and gases through wide ranges of temperature and pressure. The results of our experiments with ethyl alcohol have recently been published in the 'Philosophical Transactions;' those with acetic acid in the 'Journal (Transactions) of the Chemical Society'; and the Royal Society have in their hands a similar investigation on ether. We have also finished a study of the thermal properties of methyl alcohol.

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* 'Chem. News,' Oct. 17, 1879 (vol. 40, p. 186).

In consequence of a recent publication by Wroblewski, of which we have seen only the abstract ('Deutsch. Chem. Ges. Berichte,' 1886 (Referate), p. 728), we deem it advisable to communicate a short notice of an investigation in which we are at present engaged.

We find that with the above-mentioned substances, acetic acid excepted, whether they are in the liquid or gaseous state, provided volume be kept constant, a simple relation holds between pressure and temperature. It is

$$p = b\mathrm{T} - a.$$

This is evidently a simple modification of Boyle's and Gay-Lussac's laws; for at low pressures, where volume is large, the term a approaches and finally equals zero,' while b diminishes and finally becomes equal to the value of c, calculated from the ordinary equation,

$$p = \frac{c\mathrm{T}}{v}.$$

We have as yet only had time to apply this formula with ethyl ether to the liquid state; and as we are not yet quite certain whether the relation holds when 1 gram of ether occupies volumes between 4 and 20 c.c., we are at present engaged in measurements of volumes and pressures at temperatures between 220° and 280°. Assuming the above relation to be true (and it is at all events a close approximation to truth), it is possible to calculate those portions of isothermals included within the liquid-gas area, and represented in Andrews' diagram by horizontal straight lines. We have calculated a few of these isothermals for ether, and find that the areas above and below the horizontal lines (see woodcut) are equal, when measured by a planimeter.

Reserving a full discussion of the subject until the completion of our experiments, we would here point out the similarity between the equation $p = b\mathrm{T} - a$, and those proposed by Clausius and by van der Waals to represent these relations. Clausius's formula is

$$p = \frac{\mathrm{RT}}{v-\alpha} - \frac{c}{\mathrm{T}(v+\beta)^2},$$

and van der Waals'

$$p = \frac{\mathrm{RT}}{v-b} - \frac{a}{v^2}.$$

In these formulæ Clausius's α and c are equivalent to van der Waals' b and a respectively, but R has a different signification.

We find that a somewhat similar formula agrees better with experiment than either of the above; it is

$$p = \frac{\mathrm{RT}}{v-b} - \frac{a}{\mathrm{T}v^2},$$

where R, b, and a have the same meaning as in van der Waals' formula. This formula expresses the results of experiments with great accuracy, where the volume of 1 gram of ether occupies not less than 25 c.c.; but at smaller volumes it ceases to represent the facts.

It is to be noticed that both Clausius's equation and ours introduce

T into the denominator of the second term; they evidently differ from our first equation $p = b\mathrm{T}-a$, in which a is independent of temperature.

We shall soon be in a position to communicate the results of this investigation, giving full data.

[January 18th, 1887.—We have alluded to Clausius's formula, $p = \frac{\mathrm{RT}}{v-a}-\frac{c}{\mathrm{T}(v+\beta)^2}$; his latest published formula is, however, $p = \frac{\mathrm{RT}}{v-a}-\frac{c}{\Theta(v+\beta)^2}$, where $\Theta = a\mathrm{T}^n-b$. As the second term is here also a function of temperature, it is evident that his last equation is also not in accordance with the simple relation $p = b\mathrm{T}-a$].

III. "Note on Lepidodendron Harcourtii and L. fuliginosum (Will.)." By W. C. Williamson, LL.D., F.R.S., Professor of Botany in the Owens College and in the Victoria University. Received November 27, 1886.

In March, 1832, the late Mr. Witham read to the Natural History Society of Newcastle-upon-Tyne the first public notice of the classic specimen of Lepidodendron known as *Lepidodendron Harcourtii*. Still later (1833) he published further figures and descriptions of the same specimen in his work on 'The Internal Structure of Fossil Vegetables.' Additional figures and descriptions of the same object appeared in the second volume of Lindley and Hutton's 'Fossil Flora,' and in Brongniart's 'Végétaux Fossiles.' But notwithstanding all these publications the exact plant to which they referred has long been doubtful. I hoped to have found either the original specimen in the museum of the Yorkshire Philosophical Society or the sections described by Lindley and Hutton in that of the Newcastle Society; but, though carefully sought for, I long failed to discover either one or the other.

In 1871, I laid before the Royal Society my memoir, Part II, "On the Organisation of the Fossil Plants of the Coal-measures," in which I figured (Plate 25, fig. 12), a plant that seemed to me to be identical with *L. Harcourtii;* and in Part XI (1880) of the same series of memoirs, I gave further representations of the same plant (Plate 51, fig. 10; Plate 49, fig. 11). Since the publication of the latter memoir I have obtained a fine series of specimens, which appeared to me to approach still more closely to the various representations of *Lepidodendron Harcourtii*, referred to above, and which inclined me to think that I had hitherto included two species under a common name. The two forms unmistakably belong to a common type, to which I have frequently had occasion to refer as "the type of *L. Harcourtii*," characterised by the possession of a very distinct parenchymatous medulla, surrounded by a sharply-defined non-exogenous vascular zone—the *Étui médullaire* of Brongniart—and by the *almost* entire absence of any exogenous vascular zone; the chief exception to the last feature being represented in the Plate 49, fig. 11, referred to above.

One of the most characteristic features seen in my new specimens occurs in the structure of the foliar bundles. These have been large, and in transverse sections of a stem they are rendered increasingly conspicuous, by the disappearance of a considerable amount of cellular tissue which originally belonged to them, but which is now only represented by a clear vacant space. What remains of these bundles is equally characteristic. In each case the bundle appears to be a double one; owing to the preservation not only of its vascular or xylem part, but also of a distinct and separate string of what has obviously been a modification of the hard bast of the phloem part of the bundle. A further feature occurs in the almost invariable disappearance of the entire inner cortical zone.

Visiting York a few weeks ago, I made a fresh search for Harcourt's original specimen, and with the kind aid of the officers of the museum I was this time successful. The specimen represented on Plate 98 (fig. 1), of the 'Fossil Flora,' vol. 2, was discovered, and by the kindness of Mr. Reed, the intelligent Honorary Curator of the geological department of this museum, I have been permitted to obtain a transverse section of it. It is now certain that my more recently acquired specimens represent the true *L. Harcourtii*, and, in all probability, fig. 9 in Plate 52 of my Part XI is a very young branch of the same species. Those previously figured by me and referred to above are certainly distinct. They are characterised by the greater uniformity in the composition of the entire cortex, the inner part of which is preserved, and by the absence of the duplex structure of the foliar bundle. The small size of the cells of the inner cortex, and the dense aspect both of it and of the foliar bundles (see fig. 10, Plate 51, Part XI), give to transverse sections of the form so sooty an aspect contrasted with the luminous semitransparency of the true *L. Harcourtii*, that I propose to recognise the former as *Lepidodendron fuliginosum.*

IV. "On the Organisation of the Fossil Plants of the Coal-measures. *Heterangium Tiliæoides* (Will.) and *Kaloxylon Hookeri*." By Professor W. C. WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College and in the Victoria University. Received December 1, 1886.

(Abstract.)

Several years ago the author discovered the stems and branches of a remarkable plant in the Carboniferous beds at Burntisland, which he described in the volume of the 'Philosophical Transactions' for 1873, under the name of *Heterangium Grievii*. This plant displayed a central axis, in which was combined a curious mixture of cells and vessels. These were surrounded by a vascular zone, with medullary rays, evidently a product of an investing cambium layer. Outside this exogenous growth were a complex series of cortical layers, with various arrangements of vascular bundles going off to supply lateral appendages. The indefatigable industry of my valuable auxiliaries, William Cash, Esq., and Mr. Binns, of Halifax, have supplied a series of specimens which the author soon found to be a new species of Heterangium, to which he gives the name of *Heterangium Tiliæoides*. Whilst the plant exhibits all the features of interest seen in *H. Grievii*, it has others peculiar to itself. Its central axis corresponds closely with that of *H. Grievii*. Its exogenous vascular or xylem zone is more fully developed than in the older species, but the most striking features are seen in new structures external to and developed by what has been a cambial zone. The xylem consists of groups of vascular laminæ, the inner ends of each of which groups so converge as to separate the vascular ring into a series of distinct bundles. These are not only separated from each other by primary medullary rays, but as each of these latter passes outwards towards the cortex, it rapidly expands laterally, assuming, in transverse sections, a trumpet-shaped contour. These are in fact true primary phloem rays, as fine as those seen in the shoots of the Lime tree; hence the specific name of *Tiliæoides* given to the plant by the author.

Between these large phloem rays are clusters of phloem, the radial diameter of each of which is co-extensive with the xylem part of the bundle to which it belongs. Each of the xylem-bundles is subdivided into minor groups of two or three vascular laminæ, separated by secondary medullary rays, each of which latter can be traced as secondary phloem rays passing radially outwards through the phloem portion of each bundle. The vessels seen both in the vasculo-medullary axis and in the exogenous zone are chiefly furnished with bordered

pits, not confined, as in Conifers, to the sides of the vessels in contact with the medullary rays, but covering their entire circumference.

On examining tangential and radial sections of the phloem, it is found to contain numerous long narrow tubes, but which cannot be absolutely identified with either sieve tubes or with sclerous fibres. In the outer bark numerous flat plates of coarse sclerous parenchyma pass horizontally outwards. This feature is equally characteristic of *Heterangium Grievii*. Various forms of lateral outgrowths are also described.

Further observations are recorded on the structure of *Kaloxylon Hookeri*, also originally described by the author from specimens obtained from collieries near Oldham, in 'Phil. Trans.,' vol. 166 (Part I, 1876). These specimens, though revealing the elegant arrangements, especially of its vascular structures, to which the plant owes its name, did not fully reveal the true structure of its cortex. But soon after the publication of the memoir referred to, the author obtained from Halifax beautiful examples in which this defect was remedied. These new specimens exhibited within a remarkable epidermal layer, a thick cortex, always abounding in narrow vertically elongated tubes, which appear to have been either gum or resin canals. In some examples these are crowded together in remarkable numbers. But the Halifax specimens exhibited other peculiarities. In some of them the peripheral end of each of the five or six radiating vascular wedges, which are so characteristic of the plant, is furnished with a true phloem element. In the larger proportion of the Halifax specimens the radiating exogenous extensions of the central vascular axis are wholly wanting, and, whilst in most of the specimens which possess these exogenous growths no cellular parenchyma appears amongst the vessels of that central axis, specimens are described which display a gradual development of such tissue in that position. In some cases its amount almost equals the area occupied by the medullary vascular bundles. Some of the latter examples give off rootlets from the exterior of the central vasculo-cellular axis, which pass directly outwards through the bark. A further series of specimens is described, each of which contains vascular bundles, of which the transverse section is quadrangular, closely resembling the tetrarch bundles of many roots. Tracing these bundles downwards through a number of examples which diminish in size until very minute ones are reached, we find evidence that these bundles developed centrifugally instead of centripetally. Nevertheless the author inclines to the belief that these objects are true rootlets.

After pointing out the probability that the *Lyginodendron Oldhamium* previously described by him is, notwithstanding its exogenous mode of growth, a fern, of which *Rachiopteris aspera* was the foliage, the author states that Mr. Kidston has supplied him with structureless specimens preserved in shale, of *Sphenopteris elegans*, which display regular transverse ridges crossing their stems and branches, which seem to have been caused by the presence of bands of some hard substance, corresponding exactly with those seen in the outer bark of both the Heterangiums. These, at all events, are the only examples of fossil Carboniferous plants, in which structures comparable with those of the Heterangium stems have been discovered. It is not without significance that *H. Grievii* has not only been found in the Westphalian deposits of Pith Vollmond, but a German locality has furnished Professor von Weiss, of Berlin, with specimens of *Sphenopteris elegans*, having the same kind of bark as those found in Scotland. The author suggests that the Heterangiums may possibly have been ancestral forms, having exogenous stems and fern-like foliage, which may have bequeathed the former features to some of the modern Cycads, and the latter to the Ferns, the living Stangeria having retained some of the features of both.

Presents, January 6, 1887.

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January 13, 1887.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Hardinge Stanley Giffard, Lord Halsbury (Lord High Chancellor), whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Supplementary Note* on *Polacanthus Foxii*, describing the Dorsal and some parts of the Endoskeleton imperfectly known in 1881." By J. W. HULKE, F.R.S. Received December 14, 1886.

(Abstract.)

The author describes the large dorsal shield, which has been recently restored and now exhibits the grouping of the keeled and tuberculated fragments, which in their disconnected and scattered

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* See 'Phil. Trans.,' vol. 172 (1881).

condition had formerly been regarded as portions of separate scutes. This unique specimen shows Polacanthus to have possessed a more complete dermal armature than any other Dinosaur yet described. The pelvis is more fully described than was possible in 1881, and in particular the form and direction of the ischian arch shown to be different from those obtaining in the Iguanodont family.

II. "The Reputed Suicide of Scorpions." By ALFRED G. BOURNE, D.Sc., Fellow of University College, London, and Professor of Biology in the Presidency College, Madras. Communicated by Professor RAY LANKESTER, F.R.S. Received December 22, 1886.

The legend that a scorpion when placed within a ring of red-hot embers will, after making futile efforts to pass the fiery circle which surrounds it, deliberately kill itself by inflicting a wound with its sting in its own head is said to emanate from Spain, and is of considerable antiquity: it has been, moreover, attested by very high authority.

The phenomenon would, however, be so extraordinary that its occurrence has been much doubted. Did it happen, it would stand as Romanes[*] says "as a unique case of an instinct detrimental alike to the individual and to the species."

The subject has within recent years attracted a considerable amount of attention, and numerous conflicting statements based both upon incidental observations and upon definite experiments have been from time to time recorded.

Surgeon-General Bidie of Madras has described[†] an experiment where he concentrated the sun's rays with a burning glass upon the back of a scorpion, which thereupon stung itself and died. Dr. Allen Thomson[‡] also brings forward corroborative evidence as to the effect of rays of light. Mr. Gillman[‡] described experiments made with a circle of glowing charcoal in which he states that the scorpions died from their own sting. Mr. R. F. Hutchinson[§] pointed out that the animals experimented upon by Mr. Gillman died from the excessive heat. Mr. Gillman, however, subsequently pointed out that the temperature in the centre of such a circle of glowing charcoal as he used does not exceed 50° C.

Mr. Curran[§] tried the same experiment "a score of times," and

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[*] 'Animal Intelligence,' p. 225.

[†] 'Nature,' vol. 11.

[‡] 'Nature,' vol. 20.

[§] 'Nature,' vol. 21.

"never saw the phenomenon so graphically delineated by Byron." A would-be humorous anonymous correspondent writing at the same time summed up the subject thus: "Can a man pummel his own back? Can a horse kick its own belly? But the feat attributed to the scorpion, apart from its moral obliquity, is physically even more triumphant." Mr. Lloyd Morgan* tried a great many methods of tormenting scorpions with the view of inducing them to commit suicide, but in no single case did they attempt to do so.

Lastly, Ray Lankester† noticed when killing a scorpion (*Androctonus funestus*) with chloroform vapour in a glass box that it made "repeated blows with its sting in a straight-forward direction above its head," and at last one blow was so ill-directed as to cause the sting to catch under the free projecting margin of the posterior region of the cephalic shield. There was no laceration in this case, but it was evident there might have been. Lankester states that it is important to note the species observed in connexion with this question. Suicide may occur in some species, but not in others. He also suggested to me that I should ascertain whether the scorpion is 'immune' in regard to its own poison, as Fayrer states to be the case with certain poisonous snakes.

At least three species of scorpion occur here—a large black one, which is probably *Buthus Kochii*, a smaller lighter-coloured one, and a very small red one. I hope to determine these accurately later on. In the meantime I have tried a very large and varied series of experiments upon their stinging powers with the following results:—

1. There can be no doubt that it is physically possible for a scorpion to sting itself in a vulnerable place.

2. When a scorpion is placed in very unpleasant circumstances it not unfrequently lashes its tail about and causes actual penetration of the sting, as in the case observed by Dr. Bidie.

3. The poison of a scorpion is quite powerless to kill the same individual or another individual of the same or even of another species.

4. The poison is very rapidly fatal to a *Thelyphonus*, less rapidly so to a spider, and much less rapidly so to an insect.

5. Two scorpions when fighting repeatedly sting one another with little if any effect, the stronger kills the weaker by actually pulling it to pieces with its chelicerae.

6. Scorpions cannot stand even a dry temperature much above 50° C., but fall into a sort of "heat coma," and soon die if the temperature is raised.

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* 'Nature,' vol. 27.

† 'Linn. Soc. Journ., Zool.,' vol. 16.

I will briefly describe the experiments which have led to the above results

1 If a dead scorpion be taken which is quite limp and not in a state of rigour, it will be easily seen that the last four segments of the tail are about the only portions of the body, whether on the dorsal or ventral surface, where a scorpion could *not* sting itself Further, if two fighting scorpions be watched it will be seen that the extent to which the sting can be moved about is perfectly wonderful

2 I have tried the experiment with a charcoal fire, with a burning glass, and indeed a variety of other "cruel ' means of leading the scorpion to despair, and to attempt to end all by suicide, and have noticed that in lashing the tail about scorpions so treated have stung themselves To take the burning glass experiment, if the rays be concentrated on any part of the body the scorpion brings his sting there and endeavours to strike away the source of irritation (I have seen a scorpion do this from whom I had removed the whole sting some weeks previously), and sometimes the endeavours become more and more frantic, and the point of the sting catches somewhere, but the scorpion does not die unless the heat is concentrated on the back when it soon succumbs and this equally so even with the sting tied down or previously removed

3 Upon the third proposition, which, if once established, is a conclusive and positive refutation of the suicide theory, I have tried a great many experiments I have taken scorpions belonging to the various species found here, and by holding the sting between a pair of forceps and the scorpion in my hand I have pricked the scorpion in various places with the sting and *squeezed out its poison*, there is a little bleeding from the wound Then still holding the sting I have taken a cockroach and squeezed out some more of the poison into it, and always with these results the scorpion lives for days, the cockroach becomes very sluggish at once, and dies in an hour or so, it is always considerably paralysed, and cannot run away far or fast I have also used a large cricket instead of a cockroach stung in the femur of the large hind leg, that leg becomes paralysed Stung in the same place on both sides both the hind legs become useless, the animal crawls away on the two anterior pairs of legs, but otherwise appears happy Stung in the thorax it becomes quite torpid, when placed on its back it is not able to turn over These last experiments show that my method of squeezing out the poison is perfectly effective

I then proceeded to try stinging one scorpion with another, firstly, both being specimens of the same species, secondly, using different species There was always the same result, the stung individual never died from the sting, although I think that occasionally it became a trifle sluggish

4. After considerable search I procured some specimens of *Thelyphonus*, which I chose as being the nearest relatives of the scorpions. I stung these in my usual method, and in each case they died within six seconds. I then tried some spiders; they died in a few minutes when well stung. The action on cockroaches and crickets is described above. The equally rapid local but slower general action in these latter animals is probably to be explained by the very inefficient circulation of the blood in insects as compared with Arachnida. It would be an interesting experiment to sting a *Limulus*, and compare the effect with that on a crustacean, and with that on other Arachnida.

5. While keeping a quantity of scorpions together in captivity it is not difficult to induce a couple to fight. I isolated such a couple, and they fought on and off for two days, during which time each repeatedly stung the other. The scorpions face one another, and the more powerful manages to hold the other's chelæ within its own with a sort of hugging action; it then fights with its chelicerse, clawing about the region of the mouth. A strong scorpion will actually tear out the chelicera of a small one in this manner; it then proceeds to suck the blood and juices.

On another occasion I separated from one another two scorpions which had been fighting, and which had repeatedly stung one another. They lived perfectly well.

6. *Apropos* of Mr. Gillman's remarks about the actual temperature to which the scorpion is subjected in the "fiery circle," I tried this experiment. I placed a scorpion and a cockroach (for comparison) in an incubator with glass sides; I gave them a piece of wood to walk about upon, and gradually raised the temperature. At 40° C. both seemed uncomfortable, the cockroach performed a sort of licking action on all its legs and antennæ; at 45° C. the scorpion became very sluggish, and at 50° C. it was nearly dead. A large furious scorpion before the experiment, it now lay on its back and did not attempt to get up. I took it out and gave it a cold bath, and put it in a cool earthenware vessel, and it gradually (two hours) recovered. The cockroach I left in the incubator till the temperature reached 52° C.; when it was nearly dead I took it out, and it very gradually recovered.

To try the effect of a wet heat I placed a scorpion and a cockroach in water at 43° C., and they both died almost immediately, whereas they would both have lived in cold water for hours.

In conclusion, I think we can understand how various observers have been led to form conflicting opinions, but I think we may safely assert that scorpions do not commit suicide. There is not only an absence of proof that they possess any instinct to do so, but absolute proof that it is chemically impossible for them to do so—at any rate, in the species which I have examined. The immunity of the scorpion from any injurious influence after inoculation with its own poison is merely a further example of a principle already established by Sir Joseph Fayrer's experiments,* which show conclusively that the cobra poison will not affect a cobra, and will not even affect the viperine *Ptyas*

Postscript

Having received some criticisms and suggestions from Professor Ray Lankester with regard to the above record of experiments, I have made the following additional observations —

1 It appears to have been stated that inclosure in a circle of oil, or inclosure under an inverted tumbler, will cause a scorpion to commit suicide I placed a scorpion on a plate within a ring of cocoa nut oil It calmly walked through (This calls to mind Sir Kenelm Digby's experiment with a spider placed in the centre of a circle made of powdered "unicorn s horn Contrary to Sir Kenelm s prediction when this experiment was made before the Royal Society on 24th July, 1661, the spider, as recorded in the Society's register, "immediately ran out severall times repeated The spider once made some stay upon the powder ') I placed another scorpion on a plate, and round the edge a thick roll of rag dripping with kerosine oil The scorpion walked out over the rag I further daubed the scorpion with the oil it appeared uncomfortable but did nothing remarkable The experiment with an inverted tumbler was made and gave the same negative result

With regard to the method of 'squeezing out the poison In all my experiments I catch hold of the sting with a pair of forceps or in my fingers and press the poison out It is quite easy to press out a little drop of milky white fluid which has a very pungent smell resembling that of formic acid

As to the parts of the body in which two scorpions when fighting sting one another, I have observed that the sting is most usually inflicted under the front edge of the carapace, or between the joints of the great chelæ, and sometimes near the bases of the other legs I have constantly satisfied myself of the actual penetration of the sting I am also perfectly convinced that the scorpions so stung do not in any way suffer from the poison

As to the position of the artificial stinging carried out by me, I took especial care in all cases to avoid mechanical injury to the nerve ganglia Thus in my notes I find "Scorpion A made artificially to sting Scorpion B in three places under the cephalothorax, no cessation of activity" Again, "Thelyphonus A stung artificially in mid dorsal line between two abdominal somites The animal which was very active drew in his legs rigidly, and died in ten seconds" "Thelyphonus B stung in same manner with same results." Again, "Cockroach stung between abdominal terga." "Cricket stung in third leg, that leg became paralysed; leg removed with scissors, and the animal became quite active." "Gryllotalpa stung in one thigh, leg paralysed at once; stung in opposite thigh, same result. Other legs moving, turns over when placed on his back and crawls about. Stung now in thorax (dorsally), quite paralysed; moves jaws, but will not turn over."

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* 'The Thanatophidia of India'

I have made experiments with simple puncture for control—that is without introduction of scorpion poison. Using large insects for these experiments I obtained complete freedom from ill-effects when using simple puncture, whilst the same species of insects when punctured with introduction of scorpion poison were instantly paralysed and died in half an hour.

I also procured two small shore crabs. One I punctured between two joints of the great chela of one side; several drops of blood exuded, but they coagulated, and the crab remained well. The second I stung in the same place with scorpion's sting, squeezing it to ensure poisoning. The claw was immediately paralysed, and the crab gradually became torpid, and died in less than an hour.

III. "Supplementary Note on the Values of the Napierian Logarithms of 2, 3, 5, 7, and 10, and of the Modulus of Common Logarithms." By Professor J. C. ADAMS, M.A., F.R.S., Lowndean Professor of Astronomy and Geometry in the University of Cambridge. Received December 30, 1886.

In vol. 27 of the 'Proceedings of the Royal Society,' pp. 88—94, I have given the values of the logarithms referred to, and of the Modulus, all carried to 260 places of decimals.

These logarithms were derived from the five quantities a, b, c, d, e, which were calculated independently, where

$$a = \log\frac{10}{9},\ b = \log\frac{25}{24},\ c = \log\frac{81}{80},\ d = \log\frac{50}{49},\ \text{and}\ e = \log\frac{126}{125},$$

and a complete test of the accuracy of these latter calculations is afforded by the equation of condition

$$a - 2b + c = d + 2e.$$

In the actual case the values found for a, b, c, d, e satisfied this equation to 263 places of decimals.

Although this proved that the values of the logarithms found in the above paper had been determined with a greater degree of accuracy than was there claimed for them, yet I was not entirely satisfied with the result, since the calculation of the fundamental quantities had been carried to 269 places of decimals, and therefore the above cited equation of condition showed that some errors, which I had not succeeded in tracing, had crept into the calculations so as to vitiate the results beyond the 263rd place of decimals

Of course in working with such a large number of interminable decimals, the necessary neglect of decimals of higher orders causes an uncertainty in a few of the last decimal places but when due care is taken, this uncertainty ought not to affect more than two or three of the last figures

The Napierian logarithm of 10 is equal to $23a-6b+10c$, and the Modulus of common logarithms is the reciprocal of this quantity

Since the value found for the logarithm of 10 cannot be depended upon beyond 262 places of decimals a corresponding uncertainty will affect the value of the Modulus found from it

In the operation of dividing unity by the assumed value of log 10, however, the quotient was carried to 282 places of decimals

This was done for the purpose of supplying the means of correcting the value found for the Modulus without the necessity of repeating the division, when I should have succeeded in tracing the errors of calculation alluded to above, and thus finding a value of log 10 which might be depended upon to a larger number of decimal places

Through inadvertence, the values of the logarithms concerned, and the resulting value of the Modulus, were printed in my paper in the 'Proceedings' above referred to exactly as they resulted from the calculations, without the suppression of the decimals of higher orders, which in the case of the logarithms were uncertain, and in the case of the Modulus were known to be incorrect

Although it was unlikely that this oversight would lead to any misapprehension as to the degree of accuracy claimed for my results in the mind of a reader of the paper itself there might be a danger of such misapprehension if my printed results were quoted in full un accompanied by the statement that the later decimal places were not to be depended on

My attention has been recalled to this subject by the circumstance that in the excellent article on Logarithms which Mr Glaisher has contributed to the new edition of the 'Encyclopædia Britannica,' he has quoted my value of the Modulus, and has given the whole of the 282 decimals as printed in the Proceedings of the Royal Society,' without expressly stating that this value does not claim to be accurate beyond 262 or 263 places of decimals

I have now succeeded in tracing and correcting the errors which vitiated the later decimals in my former calculations, and have extended the computations to a few more decimal places The com putations of the fundamental logarithms a, b, c, d, e have now been carried to 276 decimal places, of which only the last two or three are uncertain.

The equation of condition, $a-2b+c=d+2e$, by which the accuracy of all this work is tested, is now satisfied to 274 places of decimals.

The parts of the several logarithms concerned which immediately follow the first 260 decimal places as already given in my paper in the 'Proceedings,' are as follows:—

a	05700 33668 72127 8
b	67972 72775 92889 4
c	42038 01732 39184 3
d	08865 93150 99834 1
e	01463 48349 12851 7

Whence $a-2b+c=$ 11792 89849 25533 3
and $d+2e=$ 11792 89849 25537 5
Difference = 4 2.

Also the corresponding parts of the logarithms which are derived from the above are—

	log 2	30070 95326 36668 7
	log 3	68975 60690 10659 1
	log 5	13580 59722 56777 3
	log 7	74183 10810 25196 7
Whence	log 10	43651 55048 93446 0

And the correction to the value of log 10 which was formerly employed in finding the Modulus is

— (263) 33 69426 01554 0

where the number within brackets denotes the number of cyphers which precede the first significant figure

The corresponding correction of M, the Modulus of common logarithms, will be found by changing the sign of this and multiplying by M^2, the approximate value of which is

0·18861 16970 1161

Hence this correction is

(264) 6 35513 15874 7

And finally the corrected value of the Modulus is

M = ·43429 44819 03251 82765 11289 18916 60508 22943 97005 80366
65661 14453 78316 58646 49208 87077 47292 24949 33843 17483
18706 10674 47663 03733 64167 92871 58963 90656 92210 64662
81226 58521 27086 56867 03295 93370 86965 88266 88331 16360
77384 90514 28443 48666 76864 65860 85135 56148 21234 87653
43543 43573 17253 83562 21868 25

which is true, certainly to 272 and probably to 273 places of decimals.

IV. "On the Crimson Line of Phosphorescent Alumina." By William Crookes, F.R.S., V.P.C.S. Received December 30, 1886.

In a paper which I had the honour of communicating to the Royal Society* in March, 1879, I described the phosphorescence of alumina and its various forms when under the influence of the electrical discharge *in vacuo*, in the following words:—"Next to the diamond, alumina in the form of ruby is perhaps the most strikingly phosphorescent stone I have examined. It glows with a rich, full red; and a remarkable feature is that it is of little consequence what degree of colour the earth or stone possesses naturally, the colour of the phosphorescence is nearly the same in all cases; chemically precipitated amorphous alumina, rubies of a pale reddish-yellow, and gems of the prized 'pigeon's blood' colour, glowing alike in the vacuum, thus corroborating E. Becquerel's† results on the action of light on alumina and its compounds in the phosphoroscope. . . . . The appearance of the alumina glow in the spectroscope is remarkable. There is a faint continuous spectrum ending in the red somewhere near the line B; then a black space, and next an intensely brilliant and sharp red line, to which nearly the whole of the intensity of the coloured glow is due. . . . . This line coincides with the one described by E. Becquerel as being the most brilliant of the lines in the spectrum of the light of alumina, in its various forms, when glowing in the phosphoroscope."

In 1881‡ I again returned to the subject, describing a large number of fresh experiments; and I may add that the red glow of alumina has been, off and on, a subject of examination with me since the year first named down to the present time.

In the papers above quoted I gave as accurate measurements of the alumina line as my instrumental means would then permit. I have recently had occasion to go over these measurements again in a large spectroscope of very accurate construction, and the results, I think, are sufficiently important to be worth bringing before the notice of the Royal Society.

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* 'Phil. Trans.,' Part 2, 1879, pp. 660, 661.

† 'Annales de Chimie et de Physique,' vol 57, 1859, p. 50.

‡ 'Roy. Soc. Proc.,' vol 32, pp. 206—208.

The spectrum consists firstly of an exceedingly faint and hazy pair of bands; these are too faint to measure, but they appear to be in about the same position as the bands 1986—2009, and 2031—2075 in the spinel spectrum given further on. Next is seen the characteristic crimson line of the alumina spectrum; this, when examined with a fine slit and high power eyepiece, is seen to be double, the distance apart of the components being about half the distance separating the D lines. Then come a pair of fainter and rather nebulous orange lines; beyond them is a dark space followed by a continuous spectrum extending to the green. The following are the measurements of the spectrum:—

Scale of spectroscope.	λ.	$\frac{1}{\lambda^2}$	Remarks.
10·550°	6942	2075	The first component of the double crimson line.
10·548	6937	2078	The second component of the double crimson line. These lines are nearly as sharp as the components of D.
10·450	6707	2223	Approximate centre of a narrow band, shading off at each side.
10 400	6598	2297	Approximate centre of a narrow band, shading off at each side. This band is somewhat sharper and brighter than the one at 2223.
10·360	6514	2357	Approximate commencement of the continuous spectrum which extends into the green, shading off too indefinitely to admit of measurement

The accompanying cut (fig. 1) gives the spectrum drawn to the $\frac{1}{\lambda^2}$ scale.

Fig. 1.

The Alumina Spectrum.

It is known that spinel (magnesium aluminate) phosphoresces with a red light, and shows a crimson line in its spectrum.* On examination in the high power spectroscope the spectrum of the light emitted by spinel under the radiant matter test is seen to differ from that emitted by ruby and alumina under the same test, and to closely approximate to the description given by E. Becquerel in 1859.

The spectrum first shows, in the extreme red, a faint double band, then a narrow crimson line, which, however, is not double like the alumina line, neither is it quite so bright and sharp. Four hazy red bands follow, the fourth being wider and more indistinct than the others. Here the spectrum of most spinels fades away. Sometimes, however, a spinel is seen to glow with a greenish tint; in these the spectrum is the same as the others up to this point, and there is also seen a bright concentration of light in the green.

The measurements of the spinel spectrum are given in the following table :—

Scale of spectroscope.	λ.	$\frac{1}{\lambda^2}$.	Remarks.
10·610?	7096	1986	Approximate commencement of first component of ill-defined double band.
10·595	7055	2009	Approximate end of ditto.
10·580	7017	2031	Commencement of ill-defined second component
10·550	6942	2075	End of ditto
10·515	6857	2127	Centre of sharp crimson line.
10·490	6798	2164	Approximate commencement of broad hazy band.
10·450	6707	2223	End of ditto.
10·440	6683	2239	Commencement of second component of group.
10·405	6608	2290	End of ditto.
10·400	6598	2297	Commencement of third component.
10·380	6555	2327	End of ditto. This band is somewhat sharper and brighter than the other components of this group.
10·370	6534	2342	Commencement of fourth component of ditto.
10·330	6454	2401	End of ditto.
9·730	5541	3257	Approximate commencement of luminous concentration in the green seen in some spinels.
9·440	5234	3650	Position of maximum luminosity of this concentration of light. From this point the spectrum appears to be continuous, shading off gradually towards the blue and violet.

The drawing (fig. 2) shows the spectrum, drawn to the $\frac{1}{\lambda^2}$ scale.

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* E. Becquerel, 'Annales de Chimie et de Physique,' vol. 57, p. 58; W. Crookes, 'Roy. Soc. Proc.,' vol. 32, p. 206.

Fig. 2.

Spectrum of Spinel (Magnesium Aluminate).

In the 'Comptes Rendus' for December 6th last* appears a brief note by M. de Boisbaudran, in which he announces, "*to take date, that alumina, calcined and submitted to the electrical discharge in a vacuum, has not given him a trace of red fluorescence. This fluorescence, as well as its special spectrum, shows itself brilliantly when the alumina contains* $\frac{1}{100}$*th and even* $\frac{1}{1100}$*th of* Cr_2O_3 *With the* $\frac{1}{10000}$*th part of* Cr_2O_3 *we still obtain very visible rose colour. . . . . From these observations the presence of chromium appears to be indispensable to the production of the red fluorescence of alumina.*"

This statement being opposed to all my experience, I immediately instituted experiments with a view, if possible, to clear up the mystery. I started with aluminium sulphate, which I knew to be tolerably pure, and in which ordinary tests failed to detect chromium. On ignition and testing in the usual manner in a radiant matter tube, the alumina line was brightly visible in the spectrum of the emitted light. Different portions of this aluminium sulphate were now purified by various processes for the separation of chromium. All gave as a result the absence of this impurity. The most trustworthy process being that devised by Wohler,† I used it to purify the bulk. The salt was dissolved in water, and excess of caustic potash added till the precipitate first formed redissolved. Chlorine was now passed through till no more precipitate fell down and the liquid retained a strong odour of chlorine. The whole of the chromium would now be in solution, whilst the alumina would be in the precipitate. The alumina was filtered off, well washed, and a portion tested in the radiant matter tube. It gave as good an alumina spectrum as did the original sulphate, the crimson line being very prominent.

The alumina thus purified was a second time dissolved in caustic potash and submitted to the chlorine purification. Again in the radiant matter tube the alumina gave its characteristic crimson line spectrum.

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* 'Comptes Rendus,' vol. 102, p. 1107.

† 'Select Methods in Chemical Analysis,' 2nd edition, p. 124.

The filtrate from the alumina, which should contain all the chromium present in the form of potassium chromate, was supersaturated with hydrochloric acid and boiled till free from volatile chlorine compounds. Alcohol was then added, and it was again boiled to reduce to the state of sesquichloride of chromium any chromic acid which might be present. Ammonia in excess was now added, and the whole was boiled; a very small precipitate of a brownish colour fell down; it was filtered and washed. This precipitate, which was not more than the $\frac{1}{5000}$th part of the alumina from which it was derived, contained no chromium whatever; it was too small in quantity to admit of a complete analysis being made, but all the tests which I could apply showed it to be a mixture of ferric oxide and alumina.

One part of this precipitate was mixed with 100 parts of the pure alumina from which it had just been separated, and the mixture was tested in the radiant matter tube. The phosphorescence was the same as in the two previous experiments, the crimson line being neither better nor worse.

I now prepared aluminium chromate, and tested its action in the radiant matter tube. It was almost black after ignition and refused to phosphoresce. A mixture was then made of aluminium chromate and alumina in the proportion of one part chromium to 100 parts of aluminium. After ignition the colour of the mixture was almost white. Tested, it gave very poor phosphorescence; the alumina line was faintly visible.

Aluminium acetate was mixed with 5 per cent of ammonium bichromate, ignited with sulphuric acid, and tested in the radiant matter tube. There was no phosphorescence. The same mixture was heated to a high temperature before the blowpipe, when it gave a very feeble phosphorescence, but I could detect no line in the spectrum.

Pure alumina was mixed with 5 per cent of ammonium bichromate, and moistened with sulphuric acid. After ignition it phosphoresced with a reddish colour, and the spectrum showed a concentration of light in the orange but no alumina light was visible. The tube was opened, and the contents heated to a very high blowpipe temperature. In the radiant matter tube it gave the same results as before.

A mixture of 0·5 per cent ammonium bichromate, 10 per cent of lime, and 89·5 per cent of pure alumina was ignited with sulphuric acid, and tested in the usual way. The calcium brought out a trace of yttrium and samarium bands, but no crimson line was to be seen.

Alumina precipitated from its ammoniacal solution by boiling was found to glow with a green light in the vacuum tube and to give no crimson line in its spectrum. The tube was now opened, and some of its contents removed and heated in a hot blast blowpipe to the melting point of platinum for about five minutes. Re-tested in a vacuum tube this alumina was seen to glow at the points and edges of the lumps where the heat had been fiercest, with a red light, giving a faint line spectrum. The bulk of the mass, however, gave out the original green glow.

To get the crimson line most brilliantly, it is necessary to ignite the earth to the highest temperature of the blowpipe flame. With a slightly less heat the phosphorescence is not strong, and the line is faint. When the temperature has not been raised high enough the colour of the emitted light in most aluminas is green, and no line is visible; whilst the same earth raised to a higher temperature glows with a red light, and the red line comes into view. The most brilliant crimson line, when seen at all, has always been obtained when the alumina has been kept near the melting point of platinum for some time.

Physical differences, or perhaps even difference in molecular composition, also exert a great influence on the phosphorescence of alumina. In this connexion, I ask permission to quote a sentence from my paper of May, 1881, already mentioned:—"Two earthen crucibles were tightly packed, the one with sulphate of alumina, the other with acetate of alumina. They were then exposed, side by side, to the most intense heat of a wind-furnace—a heat little short of the melting point of platinum. The resulting aluminas were then tested in the molecular stream. The alumina from the sulphate gave the crimson glow and the spectrum line. The alumina from the acetate gave no red glow or line, but a pale green phosphorescence."

Experience gained in the yttria research has taught me that the possibility of the molecule of aluminium being composed of two or more submolecules, only one of which is capable of giving the crimson line phosphorescence, must not be overlooked. To test this hypothesis alumina, as pure as I could prepare it, was submitted to three separate processes of fractionation, the operations in each case being repeated from twenty to thirty times. Alumina giving the crimson line always concentrated towards one end of the fractionations, whilst at the other end the alumina sometimes phosphoresced of a green tint, and at others scarcely phosphoresced at all, the crimson line being either very feeble or entirely absent in the spectrum. The earths were always ignited for the same time and, as nearly as possible, to the same temperature.

In no case could chromium be detected at either extremity of the fractionations.

These experiments are perhaps too few to permit any important inference being drawn from them. There seem, however, to be four possible explanations of the phenomena observed:—

1. The crimson line is due to alumina, but it is capable of being suppressed by an accompanying earth which concentrates towards one end of the fractionations
2. The crimson line is not due to alumina, but is due to the presence of an accompanying earth concentrating towards the other end of the fractionations
3. The crimson line belongs to alumina, but its full development requires certain precautions to be observed in the time and intensity of ignition, degree of exhaustion, or its absolute freedom from alkaline and other bodies carried down by precipitated alumina and difficult to remove by washing, experience not having yet shown which of these precautions are essential to the full development of the crimson line and which are unessential
4. The earth alumina is a compound molecule, one of its constituent molecules giving the crimson line According to this hypothesis alumina would be analogous to yttria

It is not unlikely that a chemist wishing to obtain alumina of exceptional purity might submit it to a series of operations, akin to fractionation, which would have the effect of giving earths phosphorescing either with a strong crimson line, or with little or no crimson line, and either of these samples of alumina might be looked upon by him as pure It is possible that some such explanation as this may be at the bottom of the contradictory statements respecting the crimson line of alumina

Presents, January 13, 1887

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January 20, 1887

Professor STOKES, D C L, President, in the Chair

The Presents received were laid on the table and thanks ordered for them

The Right Hon the Lord Halsbury was admitted into the Society

The following Papers were read —

I ' Some Anomalies in the Winds of Northern India, and their Relation to the Distribution of Barometric Pressure ' By S A HILL, B Sc, Meteorological Reporter to Government, North-Western Provinces and Oudh Communicated by H F BLANFORD, F R S Meteorological Reporter to the Government of India Received January 3, 1887

(Abstract)

In this paper the author points out that notwithstanding the great amount of light thrown upon the circulation of the atmosphere over Northern India by the accurate and intercomparable barometric and other observations made at numerous meteorological stations during the last thirteen years, there are still some unexplained anomalies connected with the wind system of that part of the world The most important of these anomalies are the following —

(1) The winds of the hot season not infrequently blow against a rising barometric gradient *i e* from places where the pressure is low to others where it is higher

(2) The velocity of the wind has little or no relation to the distribution of pressure, but increases and diminishes with the temperature

(3) An unusual accumulation of snow on the North west Himalaya during the winter and spring months causes, as Mr H F Blanford has shown unusually strong dry westerly winds over the plains during the succeeding summer, whereas the high pressure at sea level accompanying cold over the regions to the north of India should give rise to easterly winds

The paper is divided into three parts in which these three anomalies are discussed in order The first part is in reality a train of inductive reasoning which leads up to the hypothesis that the cause of the

anomalous hot winds is to be found, not in the distribution of pressure at the level of the plains but in an interchange between the lower atmospheric strata and those at high levels, effected through the medium of convection currents set up by the diurnal heating of the earth's surface when the sun shines This hypothesis was first put forward by Koppen to explain the diurnal inequality of wind velocity, and the author shows that in the dry season the vertical distribution of temperature in India is such that convective action capable of producing such interchange must occur

In the second part the diurnal variation of the wind velocity is attributed to convective interchange and it is shown that probably the annual inequality may be explained in the same way the barometric gradients prevailing at high levels between the plains and the mountains to the north of India being subject to an annual variation dependent on the temperature To verify the conclusions deduced from the convection hypothesis the midday pressures at 10 000 feet above sea level have been computed for the months of January May July and October from the observations of many years at forty stations and the resulting values have been laid down on a set of charts another set giving the distribution of pressure at sea level and the prevailing wind directions over India The high level distribution of pressure as shown on the charts is found to be exactly such as would produce the observed anomalies in the wind direction and velocity The charts also furnish reasons for the particular paths taken by the disturbances which bring the winter rainfall of Northern India and by the cyclonic storms originating at different seasons in the Bay of Bengal the rule being that the storm centre follows the line of lowest pressure in a stratum of the atmosphere lying above all local obstructions such as the mountain ranges in the interior of India

The third part of the paper is devoted to proving that in years when the summer rains fail the gradients for westerly winds at 10 000 feet over Northern India are intensified in the first place by the unusual cold over the North west Himalaya, due to the previous snowfall and afterwards by the great heat of the plains which have not been cooled by the usual precipitations in June and July The evidence for this conclusion is not so clear as it might be, but it is shown that when the most trustworthy observations are compared the gradients for westerly winds at 10 000 feet over the Gangetic plains were very high in the remarkably dry years 1877 and 1880 whilst they were very low that is to say there were gradients for easterly winds over a great extent of the plain, in 1879 and 1884, which were years with excessive rain In the moderately dry year 1883 there was a considerable gradient for westerly winds, but not nearly so great as in 1877 or 1880

The paper is illustrated by three plates, the first giving the sea-level distribution of pressure and the wind directions all over India, the second the distribution of pressure at a height of 10,000 feet, and the third the curves of temperature decrement on ascending, both as given by observation in Glaisher's balloon ascents, and as computed on the hypothesis of adiabatic convection.

II. "Evaporation and Dissociation. Part V. A Study of the Thermal Properties of Methyl Alcohol." By WILLIAM RAMSAY, Ph.D., and SYDNEY YOUNG, D.Sc. Communicated by Professor G. G. STOKES, D.C.L., P.R.S. Received January 6, 1887.

(Abstract.)

This is a continuation of the investigation in which the authors are engaged. The measurements include the expansion of the liquid, the pressure of the vapour, and the compressibility of the substance in the gaseous state, and from these are deduced the densities of the saturated vapour and the heats of vaporisation. The total range of temperature is from −15° to +240°, the range of pressure, from 11 mm. to 60,000 mm. The conclusions announced in their previous papers are supported by these measurements. The apparent critical temperature is 240.0°, and the critical pressure about 59,700 mm.

III. "Further Discussion of the Sun-Spot Observations made at South Kensington." By J. NORMAN LOCKYER, F.R.S. Received January 8, 1887.

In papers communicated to the Royal Society, and printed in the 'Proceedings' (vol. 31, pp. 72 and 348, vol. 32, p. 203, and vol. 33, p. 154) the sun-spot observations made at South Kensington since 1879 have been to some extent discussed.

In the last paper communicated to the Society, in May, 1886, I discussed the results obtained by the reduction of the observations of the most widened lines in the region F to *b* for the whole number of observations (700) made from November, 1879, to August, 1885.

In the latter paper it was shown that as we pass from the minimum to the maximum period included in the years named, the lines of known terrestrial elements disappear, their places being taken by lines which do not appear in any maps or tables of spectral lines. It was pointed out that such a result might be explained on the supposition that since the solar atmosphere is quietest and coolest at the minimum period, the vapours of terrestrial elements can exist then at the sun spot level and give some of their characteristic absorption lines while at the more intensely heated and much disturbed maximum sun spot period the vapours of terrestrial elements are dissociated, at the spot level and give vapours which have no terrestrial equivalents

Since last May the reduction of the observations of the most widened lines in the region *b* to D has been continued and the results are given in the present communication They strikingly confirm those previously obtained The conclusions drawn in the previous paper are therefore much strengthened by the evidence obtained from this new region

The observations referred to in the former paper were given in a series of tables corresponding tables are given in the present communication so that a clear comparison can be made Tables A and B show that for each of the elements taken—iron and titanium—the number of lines seen in the aggregate in each hundred observations decreases from the minimum to the maximum period

Nickel was given in the previous paper but in this region *b* to D Thalen only gives five lines due to nickel and only one of these is found amongst the most widened lines and that only once in the fifth hundred

Table C gives the lines recorded as most widened but not included as metallic lines by Angstrom or Thalen and it will be noticed these are recorded as most numerous at the maximum period

In the curves given in Fig 1 it is seen that there is the same rapid rise in the unknown lines to the third hundred the same gradual rise from this to the sixth hundred and then the commencement of the fall, just as in the previous paper for the other region of the spectrum under observation The curve due to iron exhibits an almost exact coincidence with the one previously published for the other region with the difference that in this part of the spectrum the number of iron lines is not so great

The titanium curve shows a sudden rise in the fifth hundred One line (5226) was amongst the most widened no less than twenty two times, and it is a singular fact that this line was only recorded as most widened on one other occasion (in the fourth hundred) in the whole 700 observations Although at first sight this seems to be a result contrary to that obtained in the case of iron it should be remarked that this line is one seen in the spectrum of the spark and may be considered to be due to high temperature, therefore its appearance in the maximum period was fully in accordance with the other facts adduced

The reductions have been made and the tables prepared by Messrs Mills, Spencer, and Taylor

TABLE A.—IRON.

Lines observed in Sun-spot Spectra at Kensington among the most Widened Lines. *b*—D.

	5191·7	5194·1	5201·5	5203·7	5207·6	5226·8	5232·1	5262·4	5265·8	5268·5	5269·5	5323·4	5327·8	5339·2	5340·2	5348·6	5352·4	5361·9	5364·0	5366·5	5369·0	5370·5	5396·1	5403·1	5404·8	5428·8	5445·9	5454·7	5496·6	5500·5	5506·9	5614·5	5761·9
1st HUNDRED. 12th November, 1879, to 29th September, 1880.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—				—	—		—	—	—	—	—	—	—	—	—	
2nd HUNDRED. 29th September, 1880, to 15th October, 1881.	—		—	—	—	—	—	—	—	—	—	—	—	—	—			—	—	—	—	—	—	—	—	—	—		—	—	—		•
3rd HUNDRED. 18th October, 1881, to 27th June, 1882.				—	—				—	—	—																						
4th HUNDRED. 1st July, 1882, to 28th August, 1883.			—	—	—					—	—																						
5th HUNDRED. 30th August, 1883, to 23rd June, 1884.				—	—					—	—														—				—	—			—
6th HUNDRED. 24th June, 1884, to 12th February, 1885.	No lines																																
7th HUNDRED. 18th February, 1885, to 24th August, 1885.	No lines.																																

TABLE B – TITANIUM

List of most Widened Lines observed at Kensington *b*—D

	5188 3	5192 3	5205 5	5209 5	5226 0	5246 3	5255 0	5267 2	5271 5	5295 5	5296 7	5396 1	5425 0	5511 8	5674 4	5688 5	5701 5	5738 0
1st hundred lines	│	│		│		│					│							
2nd hundred lines	│			│		│		│	│		│			│		│	│	
3rd hundred lines				│			│			│	│				│			│
4th hundred lines					│						│						│	│
5th hundred lines			│	│	│							│						
6th hundred lines							│											
7th hundred lines							│						│				│	

Table C.—Unknown Widened Lines Observed at Kensington

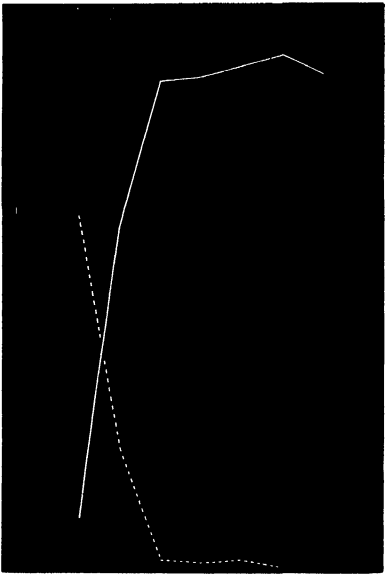
	1st hundred	2nd hundred	3rd hundred	4th hundred	5th hundred	6th hundred	7th hundred
5170 1		1					
5170 5				1			
5176 0				1			
5178 0		2					
5180 9		1					
5190 8	16	5					
5191 8		1					
5193 5							1
5199 4							1
5199 6							1
5209 2							[illegible]
5214 3	2						
5214 5						1	
5215 5	2					1	
5216 2	2					1	
5216 5		2				1	
5217 1	2	2				1	
5218 0			1			2	1
5218 5					93	6	
5218 7	1	18	2	14			
5219 0			1				[illegible]
5219 3	1	5	5				
5219 5						2	
5220 0						1	
5223 4		1					
5224 0			2	[illegible]	2	3	
5224 2	1		1			3	4
5224 6	2		2			3	
5224 8						3	4
5226 5						1	
5227 3	1						
5236 8		1					
5237 5						4	1
5238 0		1	4	[illegible]			
5240 0			1				
5246 5			1				
5255 8							2
5256 0			2				
5259 3							6
5259 5		2	4	2		3	[illegible]
5263 4	2	1					
5272 5	2						
5275 0	1						
5277 0		1					
5296 0			1				
5296 2	1	1	3	1			
5297 5	2	4	2				
5300 0		1					
5307 5		6	8				
5318 5						1	
5320 8		6	8				
5321 8		7	8				
5327 2					1		

	1st hundred	2nd hundred	3rd hundred	4th hundred	5th hundred	6th hundred	7th hundred
5327 7	14	6			1		
5328 4		1					
5328 6		1					
5329 2		1					
5331 0				1			
5345 0	1						
5351 0					1		
5357 5					1		
5364 2		1					
5393 5		1					2
5409 0		1					
5409 2					2		
5410 0		1					
5412 2							1
5413 2							1
5414 5					2		
5419 5						5	
5423 5					1		
5423 7	1						
5424 0							4
5424 5			1				
5425 3					1		3
5426 0		21	52	37	51	73	45
5426 2				1			
5427 0						1	
5428 9		1					
5431 6			6	1			
5434 5			6				
5435 5			6				
5444 0		1					
5444 2	1						
5447 0			1				
5447 3				1			
5459 0		17	26	29	24	65	18
5459 5							11
5460 0		21	48	15	21	2	10
5461 0		9	7	3	1		3
5462 5				1			
5463 3			1				
5466 0			1				
5475 0							3
5481 0		2					
5484 5							3
5486 0		1					
5489 0			5	4			
5489 5			1	3			
5492 5		1					
5493 6		1					
5505 2					1		
5505 8					1		
5511 4		1					
5513 2	1	1	1		1		
5515 5		1			1		
5529 5		3					
5531 6		1					
5532 0			1				

	1st hundred	2nd hundred	3rd hundred	4th hundred	5th hundred	6th hundred	7th hundred
5532 5			1				
5533 8		1					
5534 2		1					
5535 0					1		
5535 5					1		
5536 0				1			
5536 2		2	5	4			
5536 5						4	
5536 8						4	
5537 0		2	5				
5538 0				1			
5541 0					1		
5558 4		1					
5559 0				1			
5577 6			2				
5583 5			2				
5602 0				1			
5621 0						1	
5622 0						1	
5626 0			9	4			
5626 5		1		1			
5626 7			9				
5627 0			7	2			
5627 5			19	3			
5628 0		17	1				2
5629 5			1				
5636 2		1					
5637 5			1				
5660 0				1			
5669 5			2	5			
5669 7							1
5670 0			1	1		1	2
5670 3							3
5670 5			1	3			
5671 0		51	56	64	62	85	30
5671 5			1	1			
5672 0		51	56	64	61	83	32
5674 5			1				
5677 5				1			
5678 0				1			
5696 0							4
5697 0				4			
5698 0		1					
5698 5				1	1		4
5699 0		23	1	10			
5702 0				2			1
5703 0				4			
5706 0			1				
5719 0				2			
5721 0						3	
5721 5				1			
5722 0							3
5722 5							4
5723 0			2	6			
5724 0							1
5726 0		2	7	8	60	80	72

	1st hundred	2nd hundred	3rd hundred	4th hundred	5th hundred	6th hundred	7th hundred
5726 5		13	51	25	26		
5727 5				4			
5728 0				5			
5729 5				2			
5730 0		15	55	38	83	88	73
5730 5			1				
5731 8				1			
5732 0					1		
5733 0				1			
5734 5		1	1	6			
5735 0				1	2		11
5735 5				8		1	
5736 0				3	9	11	2
5736 5		18	6	10	13		
5736 8				1			
5738 5				[illegible]			
5741 5							6
5742 0		2	3	11	7	7	5
5742 2				1			
5742 5				17	9	6	
5743 0					1	1	
5743 5			1				
5744 5			1	6		2	
5745 0				1	1		
5746 0						3	
5747 0				8			
5747 5							2
5748 0				1			
5756 0					6		
5758 5				2			
5760 2					1		
5763 5				1			
5765 0				4			
5772 5				1			
5776 5		1		1			2
5777 5		3		7			2
5794 0					1		
5813 5			2	1			
5814 0				8			10
5816 5		1					
5817 0		1		1			
5819 6					2		
5821 0				1			
5821 5							2
5824 0				1			
5826 5				2			
5829 5				2			
5846 0			6				
5846 3		8	4				
5846 5							4
5847 5							4
5851 5		3	1				
5852 0			4				
5855 2				1	1		
5856 6					1		
5857 5							3

	1st hundred	2nd hundred	3rd hnndred	4th hundred	5th hundred	6th hundred	7th hundred
5858·4		1					
5860·0							8
5860·2							3
5862·0							12
5863·0					1		6
5863·2		5					
5864·0					2		2
5864·3						1	4
5865·0		8	16	12	1	1	23
5865·5			1	2			1
5866·0				7	10	5	50
5867·0							10
5867·5		4		14		8	10
5869·5					1		
5870·0						5	
5876·5						2	
5884·5							1
5885·2					1		
5886·5				1	1		3
5887·5				1			3
5890·0							1
5890·5							10
5893·0							1



Presents January 20, 1887

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January 27, 1887

Professor G G STOKES, D C L, President in the Chair

The Presents received were laid on the table, and thanks ordered for them

The following Papers were read —

I "On a Perspective Microscope" By GEORGE J BURCH Communicated by J RUSSELL REYNOLDS, M D, F R S Received January 7, 1887

(Abstract)

In 1874 the author discovered a form of microscope giving constant magnification along the optic axis, so that objects were shown by it in microscopic perspective

By writing (f_1+f_2+H) for the distance between two thin lenses, he obtained for the formula of the system

$$\frac{f_2(f_2+H)u-f_1f_2(f_1+f_2+H)}{Hu-f_1(f_1+H)}=v$$

u being the distance from the object to the first lens and v that from the second lens to the image

Putting $H=0$ in this equation three things result

1 dv/du which represents the longitudinal magnification, becomes constant, namely $-(f_2/f_1)^2$

2 The lateral or angular magnification, f_2/f_1 is also constant,

3 A picture of an object so magnified, drawn with the camera lucida, when viewed from a distance f_2/f_1 times less than that at which it was drawn has the perspective belonging to an object magnified $(f_2/f_1)^2$ times

The distance at which the eye must be placed is great but may be reduced by employing three lenses, the distance between the first and second being $(f_1+f_2+f_2/m)$, and that between the second and third $(f_2+f_3+mf_2)$.

If the lenses are nearly but not quite in the afocal position, greater power and a wider field may be obtained, but it is at the expense of the penetration, which may, however, with advantage be limited to the thickness of the object. The instrument offers great advantages for artistic purposes, but lenses or mirrors of specially wide angle are needed for the farther development of the invention.

The optical conditions of a system of two thin lenses at varying distance apart are shown by diagrams.

In Diagram I the u and v of the formula employed are set off as abscissæ and ordinates, and the curves (which are rectangular hyperbolas) drawn for several values of H. In the afocal position of the lenses the curve degrades into a line which is a tangent to all the hyperbolas at the point (f_1, f_2). The locus of vertices and locus of centres of these curves being straight lines, and the hyperbolas all touching the point (f_1, f_2), it is shown that the principal foci, principal points, and equivalent focal length for any given position of the lenses can be found by rule and compasses, without drawing the curve.

In Diagram II the actual position of the lenses, their principal foci, separate and combined, and the principal points, positive and negative (answering to the vertices of the curves in Diagram I), are plotted down as abscissæ, the values of H on an enlarged scale being taken as ordinates.

Diagram III shows the same for two lenses of equal focal length.

Comparison of these two diagrams suggests the employment of the term "Pseudo-Principal Points" for those positions at which the magnitude of the image is in the constant ratio f_2/f_1 to that of the object for every value of H, inasmuch as the distance from these to the principal points gives the measure of the "penetration" of the system.

II. "On the Thermodynamic Properties of Substances whose Intrinsic Equation is a Linear Function of the Pressure and Temperature." By Professor GEORGE F. FITZGERALD, M.A., F.R.S. Received January 11, 1887.

Professor Ramsay has communicated to me that he and Mr. Young have found that within wide limits several substances in the liquid and gaseous states have the following relation connecting their pressure (p), temperature (T), and specific volume (v),

$$p = a\mathrm{T} + b,$$

where a and b are functions of v only.

Now in this case the following are the forms that the thermodynamic equations assume. T is temperature, and ϕ is entropy, and c and e are functions to be investigated, c being $= d\mathrm{I}/d\mathrm{T}$, where I is the internal energy, and $e = d\mathrm{I}/dv$.

Then

$$\mathrm{T}\, d\phi = c\, d\mathrm{T} + (e + p) dv.$$

From this, as $de/d\mathrm{T} = dc/dv$, we have $dp/d\mathrm{T} = e + p/\mathrm{T}$.

But $dp/d\mathrm{T} = a$ by the intrinsic equation, and is a function of v only, $\therefore e = -b$, which is a function of v only, $\therefore de/d\mathrm{T} = 0$, $\therefore de/dv = 0$, $\therefore$ is c a function of T only, and $d\mathrm{I} = c\, d\mathrm{T} + e\, dv$ gives—

$$\mathrm{I} = \int c\, d\mathrm{T} + \int e\, dv = \gamma + \lambda,$$

where γ is a function of temperature only, and λ a function of volume only.

Similarly,

$$\mathrm{T}\, d\phi = c\, dt + a\mathrm{T}\, dv;$$

$$\therefore\ d\phi = \frac{c}{\mathrm{T}} \cdot dt + a\, dv,$$

$$\therefore\ \phi = \int \frac{c\, dt}{\mathrm{T}} + \int a\, dv,$$

$$\phi = \Gamma + \varpi,$$

where Γ is a function of temperature and ϖ of volume only.

Hence we see that c, the specific heat at a constant volume, is a function of the temperature only, and the internal energy and the entropy can be expressed as the sums of two functions, one a function of the temperature only, and the other of the volume only.

For the specific heat at constant pressure we have—

$$\mathrm{C} = c + (e + p) \frac{dv}{dt}$$

$$= c + a\mathrm{T} \cdot \frac{-a}{\mathrm{T}a' + b'},$$

where $a' = da/dv$ and $b' = db/dv$,

$$\therefore\ \mathrm{C} - c = -\frac{\mathrm{T}a^2}{\mathrm{T}a' + b'}.$$

In the case of the particular values of a and b that Professor Ramsay has suggested to me, when the intrinsic equation assumes the form—

$$p = \frac{\mathrm{RT}}{v - v_0} - \frac{\mu}{v^2},$$

and when consequently

$$a = \frac{\mathrm{R}}{v - v_0}, \qquad b = -\frac{\mu}{n},$$

we have

$$e = \mu v^{-n},$$

and

$$\mathrm{I} = \gamma - \frac{(n-1)\mu}{v^{n-1}},$$

$$\phi = \Gamma + \mathrm{R} \log (v - v_0),$$

$$\mathrm{C} - c = \frac{\mathrm{TR}^2}{\mathrm{TR} + n\mu(v - v_0)^2 v^{-n-1}}.$$

It would be most important if by some method, Kœnig's for instance, or by inserting a small microphone into a tube, the velocity of sound in substances in various states could be accurately determined, as that would enable us to determine C and c separately.

III. "On the Morphology of Birds" By Professor W. K. PARKER, F.R.S. Received January 13, 1887.

(Abstract)

Introductory Remarks.

During the time that the special study of the development of the skull has occupied my attention, the rest of the skeleton has been neglected; it has, however, had its cultivators in no small number.

In a limited degree the skeleton has been worked out by me;—for instance, the shoulder-girdle and sternum in the Vertebrata generally; in birds, the whole skeleton did at one time—a quarter of a century ago—take up much of my thought

The *development* of the skeleton, generally in this Class, is a subject of great interest, and I am anxious to catch up all the scattered results that lie before me, of the excellent but extremely limited labours of other biologists

I did begin the study of the development of the limbs, sternum, pelvis, and spine, in 1842, and some of the results will be brought forward in the present paper.

This will be, I trust, but the first-fruits of my most recent work; for, during the long years that have elapsed since this research was fairly begun, I have lost no opportunity of laying up in store embryos and young of birds of many kinds. These stores, if well worked out, will yield a series of papers like the one now offered to the Society.*

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* Although I have for many years past kept a register of the presents of

The bibliography of my published papers on the Osteology of the Thorax partly and of the Skull largely, is given in the general Bibliographical List It has been necessary to do this, as every scrap and part of the older work is wanted, now that an attempt is made to build the old and the new into something like a structure having form and fulness

There are several things that go to increase the interest in the morphology of these culminating Sauropsida at the present time

First—The discovery by Gegenbaur, Huxley, and others, of the close relationship of birds and reptiles especially of the extraordinary fact that the hind limb and pelvis of even the most minute bird pass through a stage in which they correspond almost exactly with the hind limb and pelvis of the most gigantic kinds of extinct reptiles—the Dinosaurs or Ornithoscelida

Secondly—The recent discoveries of biologists as to the composition of the Cheiropterygium in the various types of air breathing Vertebrata. It is now well known that the *five fingered hand* and the *foot with five toes* are the specialised modern representatives of hands and feet that had at least *seven* rays in their composition

And *thirdly*—the study of the development and general morphology of birds is, at the present time, of great interest,—now that we are looking to the study of *[illegible]* for some initial elucidation of the mystery as to the origin of the various types of Vertebrata

The labour of each succeeding day at this culminating Class makes it more and more impossible for me to conceive of birds as arising *direct* from the Dinosaurians, or *indeed* from *any other order or* group of reptiles

Long attention to the metamorphosis of the Amphibia has intensified this difficulty to me, for the newly transformed frog or newt appears to me to be the true counterpart of a newly hatched reptile—snake lizard, turtle, or crocodile

Each of these young creatures whether it has undergone a true metamorphosis or has been the subject of *prenatal transformation* is evidently an *imago*, although an imago that continues to grow

Now each amphibian has its own *larva*, for the larvæ of the various species have their specific differences

The thousand known species of existing Amphibia—Anurans Urodeles, and Cœcilians—and all the fishes that undergo metamorphosis, are as truly if not as remarkably distinct from each other in their larval as in their imago form—as much so as is the case in insects, or any other of those invertebrate types that are truly metamorphic

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materials for this and other parts of my work I cannot reproduce it here but must use this opportunity of thanking a host of kind friends for gifts which in abundance and variety, are somewhat embarrassing

If *many* of the existing Vertebrata are metamorphic *now*, is it not very probable that they were *all* metamorphic *once?*

The fact that we have, even now, such forms as the larval lamprey (or Ammocœte), the larvæ of Ganoids and Dipnoi, and the tadpoles of newts and frogs, *suggests to me the possibility of the existence of* huge swarms of low Proto-Vertebrata in the early ages of the inhabited planet.

If such proto-vertebrate forms existed, then it is quite supposable that a metamorphosis may, from time to time, have taken place, of this and that quasi-larval form into archaic reptile, ancestral bird, or primitive mammal

I am not afraid that anyone familiar with the development, structure, and habits of the existing Amphibia will see any difficulty in the passage of a metamorphic into a *so-called* non-metamorphic type, during time, and under the pressure of new outward conditions, —when the dilemma offered to the supposed low vertebrate was *Transform or perish*

To me it seems that the creature's necessity was Nature's opportunity; and that, during long ages, the morphological force had accumulated in *those low forms an enormous surplusage of unused* energy which, in the ripeness of time, blossomed out into this and that new and noble type

Of all the types of Vertebrata, there is none like the bird of high degree for illustrating what Professor Huxley calls "the threefold law of evolution,"* namely, overgrowth of some parts, starvation and even death of others, and fusion of parts originally distinct.

No kind of vertebrate whatever presents to the osteologist so hopeless an enigma in the adult skeleton as that of the bird; in the overgrowth of certain parts, the abortion or suppression of others, and the extensive fusion of large tracts of skeletal elements.

Hence this Class has largely acted upon the morphological mind; the "Comparative Anatomist" has, of necessity, undergone evolution *into the* "Morphologist," and the latter has had to be refined and developed into the "Embryologist."

In the bird class we meet with this remarkable phenomenon, namely, that the swiftest creatures by far that inhabit the earth have had, for the purposes of their most consummate mechanism, the greatest loss of freedom of the individual parts of the skeletal framework

Between the pigeon, on one hand, above, and the emeu, on the other, below, there are several families of related birds; but there is no direct superposition,—they are *obliquely above or below* each other.

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* See his paper "On the Application of the Laws of Evolution to the Arrangement of the Vertebrata, and more especially of the Mammalia" ('Zool. Soc. Proc.,' December 14, 1884, pp. 649–662).

Amongst the Carinatæ, which lie in the intermediate space, there is none better for the purposes of study than the common fowl, to this type I have devoted most attention, and have now worked out the limbs in as many stages as I formerly did the skull

I can now give an account of the *vertebral column* with *the ribs* and *sternum* the *limb girdles* and *limbs* from the end of the seventh day of incubation by which time the hyaline cartilage is perfect, and certain even of the bony tracts are begun

The fowl is an intermediate form between the emeu and the pigeon, but most akin to the latter I shall now confine myself to what is seen in the development of the skeleton (excluding the skull) in this medium type

The vertebral column at the end of a week's incubation is formed of hyaline cartilage, up to the end of the true sacrals the notochord is completely invested with cartilage but, behind those four segments only at the sides

The notochord has its constrictions in the middle of each centrum, and is most dilated at the intercentra

The neural arches do not nearly meet above the *atlas* is in four pieces—a superficial and an inner piece to the centrum and a pair of arch rudiments, the inner segment of the centrum becomes the *odontoid* process of the *axis*

Between the axis and the first true sacral all the vertebræ have separate ribs in the cervical region, except near the dorsal region, there are small styloid cartilages lying horizontally, which have their head or thick end, wedged in between the upper and lower transverse processes Near the dorsals they are transversely placed, and then begin to develop a descending process

The first vertebra of this stage with complete ribs becomes, by absorption of the lower part of the arch the last cervical in the adult Behind the twenty pre sacrals there are fifteen sacrals, and this series has its subdivisions

The first develops ribs (it is dorso sacral) the next three develop minute but distinct ribs, like those near the lower part of the neck, these are lumbo sacral Then come the four sacrals with no ribs, and then the seven uro sacrals the first two of which have rib bars that ossify separately, below the upper transverse processes, which latter form a complete series from the third cervical to the last free caudal segment

Of those there are five then come five more paired imperfect rudiments, clinging to the terminal part of the notochord

At the end of the 8th day there are *six* of these, with the last elongated, and the notochord projecting behind far enough for three or four more rudiments

At the end of the 10th day the vertebral chain has undergone a great change. The *atlas* is still composed of four distinct pieces of cartilages, but the ribs have become fused above and below with the transverse processes, and the notochord is now most constricted at the *intercentra*.

Besides this, in the pre sacrals, it is constricted in two places within each centrum, so that each centrum in the modern bird corresponds to three subdivisions of this axial chord.

For *two or three days* there is evidence of an archaic subdivision of the notochord into three times as many vertebral divisions as are made now in the modern bird.

In the sacral the constrictions are fewer, they are only at the intercentra, and in the middle of the centrum.

The only absolutely necessary part of the sternum is that where the sternal ribs are attached, that is a very small part, and the rest is for the attachment of the huge muscles that act upon the wings, and for the *obliqui* and *recti abdominis*.

The limb girdles are each in *three pairs of distinct cartilages*. In front, the scapula, the minute pre coracoid, the coracoid, behind the ilium, pubis and ischium, the *pre-pubis is part* of the ilium, and that has two regions the pre ilium and the post ilium.

These parts in the bird are not continuous tracts of cartilages, ossified by several centres, but are distinct, first as cartilages, then as bony tracts, those of the *shoulder* keep distinct, those of the *hip* soon coalesce.

The wings at the end of the 7th day are three-toed webbed paws, with all the digits turned inwards. The rods that compose the main part of it are composed of solid cartilage, the humerus, radius, ulna, and 1st and 2nd metacarpals have a bony sheath round their middle part, the ends of the digits and the carpals are but partly chondrified. *Five* carpal nuclei, however, can be made out, and the two proximal nuclei are known to be further subdivided, each into two, in other types, hence we can already account for *seven* carpals in the bird, which has only *two* in the adult, in a free state.

Moreover, the 1st digit has two, and the 2nd three phalanges, the normal number, as in lizards, the 3rd, which should have four, but in birds has as a rule only one, has now two, as in the ostrich, and a few other birds, there is no sign at the end of the 7th or even of the 8th day of incubation of any more than three digits, but we have in the wrist an *intermedio radiale*, a *centrale ulnare*, and three distal carpals, answering to the three developed metacarpals. The digits up to the end of the 8th day are rounded and flattish, and are quite like those of a young newt or frog. But in two days more, at the end of the 10th day, the wing has almost acquired the adult form, and one more bony centre, that of the 1st metacarpal, has appeared. The overgrowth of the 2nd distal carpal and the 2nd metacarpal, with its large and dilated digit, has arrested the distal carpal of the 1st or short digit, the "pollex" This is the last nucleus to chondrify. It is still a very small, limpet like disk of cartilage, and is now only to be seen on the *flexor face* of the manus, inside the top of the 2nd metacarpal; the distal carpal of the 3rd ray is also small as compared with the large crescentic 2nd distal nucleus. It is thrown on to the ulnar or outer side of the manus, by the overgrowth of the middle rod and its carpal. The curve of the digits at their end is now, not inwards, or to the radial side, but outwards; and the two developed distal segments form now the core of two claws, that of the first, or pollex, being of considerable length.

Thus, by the end of the 10th day, the reptilian type of fore-foot has been attained, and the amphibian type lost; whilst the limb as a whole is now a fore leg no longer, but a *wing*, thoroughly specialised by evolutional transformation.

The fore limb has not simply become modified into a wing by the shortening of the pollex and 3rd ray, the enlargement of the 2nd, and the abortion of the 4th and 5th of a fore paw, like that of the lizard; but we have now the *historical representatives* of three more rays which have cropped up since the end of the 6th day.

I have repeatedly noticed that aborted parts, like overshadowed plants, are late to appear, and soon wither, or are arrested in their growth. This is the case here, for the new rays are late, small, and scarcely functional in the fullest development. They are not lost, however, but, like certain larval structures to be found in the skulls of the highest types of birds, they are built up into the finished wing, although they form an unimportant part of it as far as *function* goes.

The first of these additional rays is the 'pre-pollex,' this is a lunate tract of fibro cartilage attached to the inner face of the 1st metacarpal. The other two are composed of true hyaline cartilage, and appear, one on the ulnar side of the 2nd, and the other on the ulnar side of the 3rd developed metacarpal.

I have described them as *intercalary* metacarpals, for they seem to be the starved twins of the 2nd and 3rd large rays; each distal carpal, very probably, in the archaic forms carried two rays. Thus there is supposed, for such a fore limb, a digit inside the pollex of the modern bird, and then two pairs of rays, of which only the *inner* in each case has been retained.

The paddle of *Ichthyosaurus* shows this kind of primitive cheiropterygium, admirably.

Thus we can account for seven carpals and six digits in the wing of the modern bird; in the legs the specialisation is not so intense, but is very great; the study of the embryonic stages shows in it many parts that the adult bird gives no signs of whatever.

Instead of there being even two tarsals, free and functional, there is only one, and that has merely the function of a "sesamoid," and has been mistaken continually for a bone of that sort; that nucleus answers to our *naviculare*, morphologically termed the "centrale."

Notwithstanding the extreme diversity in the habits of existing birds, and the great difference seen in their shank bone, this part is always single, although composed of three metatarsals. As in reptiles, the joint at this part is not between the astragalus and tibia, as in mammals, but through the tarsal series; no sign of this structure is seen in the adult bird. That which appears to be the condyloid end of the tibia is a row of tarsal bones, the tibiale, fibulare, and intermedium; these have long been known as separate bones in young birds, but their distinctness in the early embryo as cartilaginous nuclei has only lately been made out.

I have been able, however, to demonstrate this repeatedly in different kinds of birds. The centrale also, although seen in the embryo as one of the tarsal series, was not properly identified; it is a constant element, but becomes degraded

The distal series of tarsals exists as a single tract of cartilage, and then as a single plate of bone. But it is related to three metatarsals, and the middle or thick part is the first to chondrify in the embryo, and to ossify in the chicken or young bird; there are here three *connate* nuclei, with very slight signs of distinctness. The whole mass answers to our middle and external "cuneiform bones," and to the inner half of the "os magnum." Thus five tarsals can be always made out clearly, and two more accounted for.

The 1st metatarsal, which has been known, for some time, through the valuable researches of Morse, to have occasionally a proximal as well as a distal rudiment, has, I find, *always* a proximal rudiment as well.

Then, as Dr. G. Baur and Miss A. Johnson have shown, there is a 5th metatarsal; it is a small pisiform cartilage, which soon coalesces with the 4th, and with the great distal tarsal. I can only find a "pre-hallux" by turning to Teratology, and this is not the lawful method.

There may, however, be some "reversion" or "atavism" in the polydactyle foot of the Dorking fowl, which has a well developed "pre-hallux" and a double "hallux;" the twin digits of that part have a very ichthyosaurian appearance.

Presents, January 27, 1887

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"On the Computation of the Harmonic Components of a Series representing a Phenomenon recurring in Daily and Yearly Periods." By Lieut.-General R. Strachey, R.E., F.R.S. Received April 15,—Read May 13, 1886.

1.

Following the notation commonly used, the general expression for the harmonic components of the successive terms of a series representing a periodically recurring phenomenon, observed at equal intervals of time, is—

$$a_n = p_0 + p_1 \cos nz + q_1 \sin nz + p_2 \cos 2nz + q_2 \sin 2nz + \&c.,$$

where a_n is the observed value of any term in question,

z is the angular equivalent of the time interval between the observations;

n is the number of intervals from the commencement of the period to the time when the term a_n occurs;

p_0 is the mean value of all the terms for the whole period

Then if A_n represents the sum of the terms in the above series which involve nz,

B_n the sum of the terms involving $2nz$,

C_n " " $3nz$, &c.,

$$a_n = p_0 + A_n + B_n + C_n + \&c.$$

Computation for a Daily Period

2.

For a daily period of 24 hourly intervals $z = 15°$, and consequently,

$$A_n = A_{n+24};$$

$$B_n = B_{n+12} = -B_{n+6} = -B_{n+18};$$

$$C_n = C_{n+8} = C_{n+16} = -C_{n+4} = -C_{n+12} = -C_{n+20};$$

$$D_n = D_{n+6} = D_{n+12} = D_{n+18} = -D_{n+3} = -D_{n+9} = -D_{n+15} = D_{n+21};$$

whence, disregarding the terms involving multiples of z greater than $4nz$,

$$d_n = a_n - a_{n+12} = 2(A_n + C_n),$$

and

$$\theta_n = d_n - d_{n+4} + d_{n+8} = 2(A_n - A_{n+4} + A_{n+8}) + 2(C_n - C_{n+4} + C_{n+8});$$

but

$$A_n = p_1 \cos(n \,.\, 15^\circ) + q_1 \sin(n \,.\, 15^\circ),$$

$$A_{n+4} = p_1 \cos(n \,.\, 15^\circ + 60^\circ) + q_1 \sin(n \,.\, 15^\circ + 60^\circ),$$

$$A_{n+8} = p_1 \cos(n \,.\, 15^\circ + 120^\circ) + q_1 \sin(n \,.\, 15^\circ + 120^\circ),$$

and therefore

$$A_n - A_{n+4} + A_{n+8} = 0.$$

Wherefore

$$\theta_n = 2(C_n - C_{n+4} + C_{n+8}) = 6C_n; \qquad \text{and} \qquad C_n = \tfrac{1}{6}\theta_n,$$

and $$A_n = \tfrac{1}{2}d_n - \tfrac{1}{6}\theta_n.$$

In like manner,

$$S_n = a_n + a_{n+12} = 2(p_0 + B_n + D_n),$$

and

$$\Sigma_n = S_n - S_{n+6} = 2(B_n - B_{n+6} + D_n - D_{n+6}) = 4B_n; \text{ and } B_n = \tfrac{1}{4}\Sigma_n,$$

also

$$\sigma_n = S_n + S_{n+6} = 2(p_0 + B_n + B_{n+6} + D_n + D_{n+6}) = 2(p_0 + 2D_n),$$

and

$$\sigma_{n+3} = S_{n+3} + S_{n+9} = 2(p_0 - 2D_n),$$

whence

$$\psi_n = \sigma_n - \sigma_{n+3} = 8D_n; \qquad \text{and} \qquad D_n = \tfrac{1}{8}\psi_n.$$

The successive values of A, B, C, and D, thus obtained will give with a considerable degree of accuracy, the p q coefficients, and the entire series of harmonic components of the observed quantities. It will at once be seen that—

$$A_0 = p_1;\ B_0 = p_2;\ C_0 = p_3;\ D_0 = p_4.$$

$$A_6 = q_1;\ B_3 = q_2;\ C_2 = q_3;\ D_1 + D_2 = 2 \sin 60 q_4 = \tfrac{7}{4}q_4, \text{ nearly.}$$

and
$$\left.\begin{array}{ll} p_1 = \tfrac{1}{2}d_0 - \tfrac{1}{6}\theta_0; & q_1 = \tfrac{1}{2}d_6 + \tfrac{1}{6}\theta_2. \\ p_2 = \tfrac{1}{4}\Sigma_0; & q_2 = \tfrac{1}{4}\Sigma_3. \\ p_3 = \tfrac{1}{6}\theta_0 = \tfrac{1}{6}(d_0 - d_4 + d_8); & q_3 = \tfrac{1}{6}\theta_2 = \tfrac{1}{6}(d_2 - d_6 + d_{10}). \\ p_4 = \tfrac{1}{8}(\sigma_0 - \sigma_3); & q_4 = \tfrac{1}{16}(\sigma_1 + \sigma_2 - \sigma_4 - \sigma_5). \end{array}\right\} \text{(1.)}$$

3.

The equations involving the harmonic coefficients, arising from the series of observed quantities, are usually solved according to the method of least squares; and, writing Δ_1 for (d_1-d_{11}), and δ_1 for (d_1+d_{11}), and so on, the resulting values of $p\ q$ thus obtained are as follows :—

$$\left.\begin{aligned}
p_1 &= \tfrac{1}{12}\{d_0+\Delta_1 \sin 75+\Delta_2 \sin 60+\Delta_3 \sin 45+\Delta_4 \sin 30 \\
&\qquad\qquad +\Delta_5 \sin 15\} \\
q_1 &= \tfrac{1}{12}\{d_6+\delta_1 \sin 15+\delta_2 \sin 30+\delta_3 \sin 45+\delta_4 \sin 60 \\
&\qquad\qquad +\delta_5 \sin 75\} \\
p_2 &= \tfrac{1}{12}\{\Sigma_0+(\Sigma_1-\Sigma_5) \sin 60+(\Sigma_2-\Sigma_4) \sin 30\} \\
q_2 &= \tfrac{1}{12}\{\Sigma_3+(\Sigma_1+\Sigma_5) \sin 30+(\Sigma_2+\Sigma_4) \sin 60\} \\
p_3 &= \tfrac{1}{12}\{\theta_0+(\theta_1-\theta_3) \sin 45\} \\
q_3 &= \tfrac{1}{12}\{\theta_2+(\theta_1+\theta_3) \sin 45\} \\
p_4 &= \tfrac{1}{12}\{\psi_0+(\psi_1-\psi_2) \sin 30\} \\
q_4 &= \tfrac{1}{12}(\psi_1+\psi_2) \sin 60.
\end{aligned}\right\} \quad (2.)$$

To these may be added

$$\begin{aligned}
p_5 &= \tfrac{1}{12}\{d_0+\Delta_1 \sin 15-\Delta_2 \sin 60-\Delta_3 \sin 45+\Delta_4 \sin 30 \\
&\qquad\qquad +\Delta_5 \sin 75\}, \\
q_5 &= \tfrac{1}{12}\{d_6+\delta_1 \sin 75+\delta_2 \sin 30-\delta_3 \sin 45-\delta_4 \sin 60+\delta_5 \sin 15\}, \\
p_6 &= \tfrac{1}{12}\{\Sigma_0+\Sigma_4-\Sigma_2\}, \\
q_6 &= \tfrac{1}{12}\{\Sigma_1-\Sigma_3+\Sigma_5\}, \\
p_7 &= \tfrac{1}{12}\{d_0-\Delta_1 \sin 15-\Delta_2 \sin 60+\Delta_3 \sin 45+\Delta_4 \sin 30 \\
&\qquad\qquad -\Delta_5 \sin 75\}, \\
q_7 &= \tfrac{1}{12}\{-d_6+\delta_1 \sin 75-\delta_2 \sin 30-\delta_3 \sin 45+\delta_4 \sin 60 \\
&\qquad\qquad +\delta_5 \sin 15\}, \\
p_8 &= \tfrac{1}{12}\{(\sigma_0+\sigma_3)-(\sigma_1+\sigma_2+\sigma_4+\sigma_5) \sin 30\}, \\
q_8 &= \tfrac{1}{12}\{(\sigma_1+\sigma_4)-(\sigma_2+\sigma_5)\} \sin 60.
\end{aligned}$$

4.

The values of the $p\ q$ coefficients may, however, be obtained otherwise, in a form which is somewhat simpler for computation, and not sensibly less accurate.

Since $$A_n = p_1 \cos n15^\circ + q_1 \sin n15^\circ,$$

$$A_0 = p_1, \qquad \text{and} \qquad A_6 = q_1,$$

and putting $[A_1, A_5]$ for the sum of the series of quantities A_1 to A_5 and $[\cos 75, \cos 15]$ for the sum of $\cos 75+\cos 60+\cos 45+\cos 15$ it follows that

$$[A_1, A_5] = p_1[\cos 75^\circ, \cos 15^\circ]+q_1[\sin 15^\circ, \sin 75^\circ],$$

$$[A_7, A_{11}] = -p_1[\cos 15^\circ, \cos 75^\circ]+q_1[\sin 75^\circ, \sin 15^\circ],$$

and

$$A_0+[A_1\ A_5]-[A_7, A_{11}] = p_1\{1+2[\sin 15^\circ, \sin 75^\circ]\} = 7{\cdot}5956p_1,$$

$$A_6+[A_1, A_5]+[A_7, A_{11}] = q_1\{1+2[\sin 15^\circ, \sin 75^\circ]\} = 7{\cdot}5956q_1$$

In like manner,

$$B_0+[B_1+B\ ]-[B_4+B_6] = p_2\{1+2[\sin 30+\sin 60]\} = 3{\cdot}732p_2$$

$$B_3+[B_1+B_2]+[B_4+B_5] = q\ \{1+2[\sin 30+\sin 60]\} = 3{\cdot}732q_2$$

$$C_0+C_1-C_3 = p_3\{1+2 \sin 45^\circ\} = 2{\cdot}4142p_3,$$

$$C_1+C_2+C_3 = q_3\{1+2 \sin 45^\circ\} = 2{\cdot}4142q_3,$$

$$D_0+D_1-D_2 = p_4\{1+2 \sin 30\} = 2p_4,$$

$$D_1+D_2 = q_4\ \ 2 \sin 60 = 1{\cdot}7321q_4$$

Substituting for A, B, C, D, their values in terms of d, Δ, δ, &c., we have—

$$7{\cdot}5956p_1 = \tfrac{1}{2}\{d_0+[\Delta_1, \Delta_5]+\tfrac{1}{3}(\theta_0+\theta_1-\theta_3)\},$$

$$7{\cdot}5956q_1 = \tfrac{1}{2}\{d_6+[\delta_1, \delta_5]-\tfrac{1}{3}(\theta_1+\theta_2+\theta_3)\},$$

$$3{\cdot}732p_2 = \tfrac{1}{4}\{\Sigma_0+[\Sigma_1+\Sigma_2]-[\Sigma_4+\Sigma_5]\},$$

$$3{\cdot}732q_2 = \tfrac{1}{4}\{\Sigma_3+[\Sigma_1+\Sigma_2]+[\Sigma_4+\Sigma]\},$$

$$2{\cdot}4142p_3 = \tfrac{1}{6}\{\theta_0+\theta_1-\theta_3\} = \tfrac{1}{6}\alpha,$$

$$2{\cdot}4142q_3 = \tfrac{1}{6}\{\theta_1+\theta_2+\theta_3\} = \tfrac{1}{6}\beta,$$

$$2p_4 = \tfrac{1}{3}\{\psi_0+\psi_1-\psi_2\},$$

$$1{\cdot}7321q_4 = \tfrac{1}{3}\{\psi_1+\psi_2\}$$

And

$$\left.\begin{aligned}
p_1 &= \cdot06583\{d_0+[\Delta_1,\Delta_5]+\tfrac{1}{3}\alpha\},\\
q_1 &= \cdot06583\{d_6+[\delta_1,\delta_5]-\tfrac{1}{3}\beta\},\\
p_2 &= \cdot06699\{\Sigma_0+[\Sigma_1+\Sigma_2]-[\Sigma_4+\Sigma_5]\},\\
q_2 &= \cdot06699\{\Sigma_3+[\Sigma_1+\Sigma_2]+[\Sigma_4+\Sigma_5]\},\\
p_3 &= \cdot06904\{\theta_0+\theta_1-\theta_3\} = \cdot06904\{[d_0+\Delta_1]-[\Delta_3,\Delta_5]\} = \alpha,\\
q_3 &= \cdot06904\{\theta_1+\theta_2+\theta_3\} = \cdot06904\{[\delta_1,\delta_3]-[\delta_5+d_6]\} = \beta,\\
p_4 &= \tfrac{1}{16}\{\psi_0+\psi_1-\psi_2\} = \cdot0625\{\psi_0+\psi_1-\psi_2\},\\
q_4 &= \cdot07217\{\psi_1+\psi_2\}.
\end{aligned}\right\}\ (3.)$$

The expressions before given for p_6, q_6, and p_8, q_8, are very readily computed, the multiplier for q_8, $\frac{1}{12}\sin 60 = \cdot07217$, being the same as that for q_4.

The computation of p_5, q_5, and p_7, q_7, may be rendered somewhat easier as follows:—

$$p_5 = \tfrac{1}{12}(2d_0+\Delta_4)+\cdot102(\Delta_1+\Delta_5)-p_1,$$

$$p_7 = \tfrac{1}{12}(2d_0+\Delta_4)+\cdot059(\Delta_1+2\Delta_3-\Delta_5)-p_1,$$

$$q_5 = \tfrac{1}{12}(2d_6+\delta_2)+\cdot102(\delta_1+\delta_5)-q_1,$$

$$q_7 = -\tfrac{1}{12}(2d_6+\delta_2)+059(\delta_1-2\delta_3-\delta_5)+q_1.$$

5.

Assuming that the probable errors in the observed quantities are all equal, and that (e) represents the error in a pair of observations combined (corresponding to the quantities (d) and (s)), then the probable error of all the p, q coefficients calculated by the formulæ (2) will be $\frac{1}{12}\surd(6)\,.\,e = \cdot204\,e$.

The probable errors, calculated in the manner now proposed from equations (3), will be,—

Probable error of p_1 or $q_1 = \cdot06583\surd(12)\,.\,e = \cdot238\,e$,

„ p_2 or $q_2 = \cdot06699\surd(10)\,.\,e = 212\,e$,

„ p_3 or $q_3 = \cdot06904\surd(9)\,.\,e = 207\,e$,

„ $p_4 = \frac{1}{16}\surd(12)\,.\,e = 216\,e$,

„ $q_4 = 07217\surd(8)\,.\,e = 204\,e$.

The results obtained by the two methods of calculation will therefore have no sensible difference of accuracy, and as the method now proposed is believed to be both simpler and less liable to arithmetical error it may without objection be preferred. The preliminary computations on both systems, consisting of combinations of the observed quantities by addition and subtraction, are identical up to a certain point, but the formulæ (2) involve more frequent use of tables, and greater chance of error in algebraical signs, in the final operations. It may be added that much additional labour is often needlessly created by employing the hourly differences from the mean value, instead of the hourly values themselves, which are obviously sufficient for the computation of the coefficients.

The probable errors of the values of the coefficients obtained from the equations (1), will be sensibly larger than those above stated, and on the same assumptions will be as follows:—

$$
\begin{aligned}
&\text{For } p_1 \text{ and } q_1 = \tfrac{1}{3}\surd(3)\,.\,e = {\cdot}577e,\\
&\quad ,,\ p_2 \text{ and } q_2 = \tfrac{1}{4}\surd(2)\,.\,e = {\cdot}371e,\\
&\quad ,,\ p_3 \text{ and } q_3 = \tfrac{1}{6}\surd(3)\ \ e = 289e,\\
&\quad ,,\ p_4 \qquad\quad = \tfrac{1}{4}\ e \qquad\ = 250e,\\
&\quad ,,\ q_4 \qquad\quad = \tfrac{1}{7}\surd(2)\,.\,e = 202e
\end{aligned}
$$

The probable error of a pair of observations being rather less than $\frac{3}{4}$ths of that of a single observation, the greatest possible error of the coefficients thus found will only be about $\frac{4}{10}$ths of the probable error of one of the original observations, and when great precision is not aimed at the results thus obtained may suffice, and will not be found to differ materially from those got by the more tedious methods of calculation.

6.

If the original expression for the value of a_x is transformed into the series

$$a_x = p_0 + \mathrm{P}_1(\sin nx + \mathrm{T}_1) + \mathrm{P}_2(\sin 2nx + \mathrm{T}_2) + \&\text{c.},$$

it follows that $\mathrm{P}_1 = \surd(p_1{}^2 + q_1{}^2)$; $\tan \mathrm{T}_1 = p_1/q_1$, and so with all the other terms of the series

The most convenient method of computing the values of P_1 and T_1 is as follows:—

$$\log \tan \mathrm{T} = \log p - \log q;$$

from this log sin T may at once be obtained, and

$$\log \mathrm{P} = p - \log \sin \mathrm{T}.$$

The quadrant to which the angle T belongs will depend on the algebraical signs of the coefficients $p\ q$, and the following table (in which t is the angle corresponding to $+p$, $+q$) shows the cases that may arise. In it are also indicated the positions of the earliest maxima, μ_1, of the several harmonic components.

Coefficients		Value and position of T.	Position of earliest maximum of components.			
			First order	Second order.	Third order	Fourth order
$+p$	$+q$	$T = t$ 0° to 90°	$\mu_1 = 90^\circ - t$ 0° to 90°	$\mu_2 = 45^\circ - \frac{1}{2}t$ 0° to 45°	$\mu_3 = 30^\circ - \frac{1}{3}t$ 0° to 30°	$\mu_4 = 22\frac{1}{2}^\circ - \frac{1}{4}t$ 0° to $22\frac{1}{2}^\circ$
$-p$	$+q$	$T = 360 - t$ 270° to 360°	$\mu_1 = 90^\circ + t$ 90° to 180°	$\mu_2 = 45^\circ + \frac{1}{2}t$ 45° to 90°	$\mu_3 = 30^\circ + \frac{1}{3}t$ 30° to 60°	$\mu_4 = 22\frac{1}{2}^\circ + \frac{1}{4}t$ $22\frac{1}{2}$ to 45°
$-p$	$-q$	$T = 180 + t$ 180° to 270°	$\mu_1 = 270^\circ - t$ 180° to 270°	$\mu_2 = 135^\circ - \frac{1}{2}t$ 90° to 135°	$\mu_3 = 90^\circ - \frac{1}{3}t$ 60° to 90°	$\mu_4 = 67\frac{1}{2}^\circ - \frac{1}{4}t$ 45° to $67\frac{1}{2}^\circ$
$+p$	$-q$	$T = 180 - t$ 90° to 180°	$\mu_1 = 270^\circ + t$ 270° to 360°	$\mu_2 = 135^\circ + \frac{1}{2}t$ 135° to 180°	$\mu_3 = 90^\circ + \frac{1}{3}t$ 90° to 120°	$\mu_4 = 67\frac{1}{2}^\circ + \frac{1}{4}t$ $67\frac{1}{2}^\circ$ to 90°

7.

The foregoing discussion assumes that the series of quantities dealt with is truly recurrent, that is to say, that the 25th observation will be exactly coincident with the 1st, or $a_0 = a_{24}$. In fact this will rarely be the case, and it becomes necessary to ascertain what effect any non-periodic change, or want of coincidence between the beginning and end of the series will have on the values of the several $p\ q$ coefficients.

In the absence of any knowledge of the law which determines such a non-periodic change, it may be assumed to be uniform for the period over which the observations extend. Further it will be convenient to refer the change to the middle of the period, or to the mean value of the series with reference to which the periodic variations are being considered, so that the non-periodic deviations will be equal and affected by opposite signs, at equal intervals on either side of the middle of the period.

Hence assuming that $2c$ is the whole non-periodic change with its proper sign, so that $a_0 + 2c = a_{24}$, the correction of any observation a_n in order to eliminate the non-periodic change which affects it, will be $\frac{1}{12}(12-n)c$; and the corrections of the quantities d, θ, Σ, and ψ are all equal to $+c$, those of Δ being 0, and of δ being $+2c$.

The correction for the mean value p_0 will be $\frac{1}{24}c$. This is equivalent to calculating the mean for the 24 hours, by adding together half of the 1st and 25th observations and the whole of the 23 intermediate ones, and dividing the sum by 24. This mean will correspond to the middle of the series, noon. As commonly calculated the mean corresponds to half an hour before noon if the series is supposed to begin with midnight, and to half an hour after noon if it begins with 1 A.M. It is to be regretted that meteorologists have not yet adopted any uniform system in these respects.

The correction for the p q coefficients as determined from the equations (2) will be as follows :—

„ p_1, p_2, p_3, p_4 .... [illegible]

For q_1	$+\cdot633c$,	For q_5	$+\cdot112c$,
„ q_2	$+\cdot311c$,	„ q_6	$+\cdot083c$,
„ q_3	$+\cdot201c$,	„ q_7	$+\cdot064c$,
„ q_4	$+\cdot144c$.	„ q_8	$+\cdot048c$.

For the values obtained from the equations (3) they will be—*

For p_0 .... $\frac{1}{24}c$,	For q_1 ... $+10c\times\cdot06583=\cdot658c$,
„ p_1 .... $+\frac{4}{5}c\times\cdot06583$,	„ q_2 ... $+\ 5c\times\cdot06699=\cdot335c$,
„ p_2 .... $+c\times\ 06699$,	„ q_3 ... $+\ 3c\times\cdot06904=\cdot207c$,
„ p_3 .... $+c\times\cdot06904$,	„ q_4 ... $+\ 2c\times\cdot07217=\cdot144c$.
„ p_4 .... $+\frac{1}{18}c$.	

8.

As it may happen that the values of the initial observation and of that corresponding to the hour immediately after the end of a series of mean hourly values, are not known, an approximate value of $2c$ may be obtained either graphically or by calculation as follows :—

If the mean values for two or three hours preceding midnight, say from 21 hours to 23 hours, be laid down graphically, and to the right of them the mean values from 0 hours to 2 hours, the ordinate of 0 hours being made coincident with that of 24 hours, the curves drawn through the points thus fixed would, if prolonged to meet one another, be continuous if there were no non-periodic disturbance. If they are not thus continuous half the distance between them measured on the ordinate of 0 hours will be the quantity c.

---

* As the multipliers are here the same as those which enter into the expressions (3), the corrections may most conveniently be applied to the quantities within the brackets in those expressions.

The value may be obtained by calculation, perhaps with less trouble. Assuming that the third differences of the quantities (a) will vanish, when freed from the non-periodical variation; and that the corrected series of observed values immediately before and after midnight is $a_{22}-\frac{10}{12}c$, $a_{23}-\frac{11}{12}c$, $(a_{24}-c)$ or (a_0+c), $a_1+\frac{11}{12}c$, $a_2+\frac{10}{12}c$; and

$$a_{24}=\tfrac{4}{3}(a_{23}+a_1)-\tfrac{1}{3}(a_{22}+a_2)-a_0,$$

$$a_{24}+a_0=2c=\tfrac{4}{3}(a_{23}+a_1)-\tfrac{1}{3}(a_{22}+a_2)-2a_0,$$

$$c=\tfrac{1}{3}\{2(a_{23}+a_1)-\tfrac{1}{2}(a_{22}+a_2)\}-a_0.$$

It may also be noticed that when a series of mean values for several days is being dealt with, the quantity $2c$ will be the difference between the first observed value and the value for the hour immediately following the last of the series, divided by the number of complete days in the series.

Computation for a Yearly Period.

9.

The computation of the harmonic components of a yearly series of 365 daily values, or of 73 five-day mean values, such as are now usually calculated for the principal meteorological elements, would be extremely laborious if conducted in the usual manner, and it is therefore desirable to contrive some approximate method which shall not involve excessive arithmetical operations. For meteorological purposes the following mode of procedure will, it is believed, supply what is needed, and it may possibly be employed conveniently in other cases.

When the number of the terms of a series is exactly divisible by 2, 4, 6, and 8, it will be readily seen that the new method of computation proposed in the case of a daily series of 24 hourly terms, will be applicable in its general form; all that is necessary by way of modification being the introduction of suitable changes in the combination of the terms, and the adoption of other multipliers in place of those given in the equations (3).

The series of five-day means for a year consists of 73 terms, and the above-mentioned condition is therefore not directly complied with by it. It may, however, be easily transformed by interpolation into a series of 72 terms, which will comply with the conditions, in the following manner:—

If the original series of the 73 five-day means be represented by the numbers (0), (1), (2), (3), &c., ... (72), it will be apparent that the first term (0) corresponds with noon of the 3rd day of the year, the subsequent terms following each other at five-day intervals. The new interpolated series of 72 terms should begin at 0 hours of the 1st day of the year, and will be designated by the numbers 0, 1, 2, 3, &c., ... 71. Then if T_0 and T_{-1} represent, respectively, the mean for 24 hours of the first day of the year under computation, and the last day of the preceding year, the interpolated series will be—

$$0 = \tfrac{1}{2}(T_0+T_{-1}),$$

$$1 = \tfrac{1}{2}((0)+(1))+\tfrac{1}{72}((1)-(0)) = (0)+\tfrac{37}{72}((1)-(0)),$$

$$2 = \tfrac{1}{2}((1)+(2))+\tfrac{2}{72}((2)-(1)) = (1)+\tfrac{38}{72}((2)-(1)).$$

$$3 = \tfrac{1}{2}((2)+(3))+\tfrac{3}{72}((3)-(2)) = (2)+\tfrac{39}{72}((3)-(2)),$$

. . . .

$$35 = \tfrac{1}{2}((34)+(35))+\tfrac{35}{72}((35)-(34)) = (34)+\tfrac{71}{72}((35)-(34)),$$

$$36 = \tfrac{1}{2}((35)+(36))+\tfrac{36}{72}((36)-(35)) = (36),$$

$$37 = (37)+\tfrac{1}{72}((38)-(37)),$$

. . . .

$$71 = (71)+\tfrac{35}{72}((72)-(71)).$$

Moreover, for a series of 72 terms the value of s in the fundamental equation will be 5°; and designating the sums of the cosines and sines, respectively, of the successive multiples of s between s and ns by [cos 45° to 85°], and [sin 45° to 85°], and applying a similar notation to the quantities before designated by the series A, B, C, D, we shall have—

$$[A_1 \text{ to } A_{17}] = p_1[\cos 5^\circ \text{ to } 85^\circ]-q_1[\sin 5^\circ \text{ to } 85^\circ] = 10{\cdot}95188(p_1+q_1),$$

$$[A_{19} \text{ to } A_{35}] = 10{\cdot}95188(-p_1+q_1),$$

$$[B_1 \text{ to } B_8] = p_2[\cos 10^\circ \text{ to } 80^\circ]-q_2[\sin 10^\circ \text{ to } 80^\circ] = 5{\cdot}21503(p_2+q_2),$$

$$[B_{10} \text{ to } B_{17}] = 5{\cdot}21503(-p_2+q_2),$$

$$[C_1 \text{ to } C_5] = p_3[\cos 15^\circ \text{ to } 75^\circ]-q_3[\sin 15^\circ \text{ to } 75^\circ] = 3{\cdot}29788(p_3+q_3),$$

$$[C_7 \text{ to } C_{11}] = 3{\cdot}29788(-p_3+q_3),$$

$$[D_0 \text{ to } D_4] - [D_5 \text{ to } D_8] = p_4\{1+2[\cos 20^\circ \text{ to } 80^\circ]\} = 5{\cdot}75877p_4,$$

$$[D_1 \text{ to } D_8] = 2q_4[\sin 20^\circ \text{ to } 80^\circ] = 5{\cdot}67128q_4$$

Substituting for the quantities A, B, C, D, their values in terms of the series of 73 five-day means, we obtain after reduction the values of the $p\ q$ coefficients. In the reduced expressions the following symbols are employed:—

$[(0)\text{ to }(5)]+[(67)\text{ to }(72)]=\Sigma_1$ $[(0)\text{ to }(5)]-[(67)\text{ to }(72)]=\Delta_1$.

$[(6)\text{ to }(8)]+[(64)\text{ to }(66)]=\Sigma_2$ Corresponding difference $=\Delta_2$,

$[(9)\text{ to }(11)]+[(61)\text{ to }(63)]=\Sigma_3$ " " $=\Delta_3$,

$[(12)\text{ to }(17)]+[(55)\text{ to }(60)]=\Sigma_4$ " " $=\Delta_4$,

$[(18)\text{ to }(23)]+[(49)\text{ to }(54)]=\Sigma_5$ " " $=\Delta_5$,

$[(24)\text{ to }(26)]+[(46)\text{ to }(48)]=\Sigma_6$ " " $=\Delta_6$,

$[(27)\text{ to }(29)]+[(43)\text{ to }(45)]=\Sigma_7$ " " $=\Delta_7$,

$[(30)\text{ to }(35)]+[(36)\text{ to }(42)]=\Sigma_8$ " " $=\Delta_8$

$[(0)\text{ to }(3)]+[(69)\text{ to }(72)]=\Sigma_9$,

$[(5)\text{ to }(12)]+[(60)\text{ to }(67)]=\Sigma_{10}$,

$[(14)\text{ to }(21)]+[(51)\text{ to }(58)]=\Sigma_{11}$,

$[(23)\text{ to }(30)]+[(42)\text{ to }(49)]=\Sigma_{12}$,

$[(32)\text{ to }(35)]+[(37)\text{ to }(40)]=\Sigma_{13}$.

$\sigma_n=(n)+(72-n)$. $\delta_n=(n)-(72-n)$

And let $\left(\tfrac{1}{2}(\mathrm{T}_0+\mathrm{T}_{-1})-\tfrac{1}{2}((0)+(72))\right)=\mathrm{K}$,

then

$$45{\cdot}80753p_1=(1-\tfrac{1}{73})\{[\Sigma_1\text{ to }\Sigma_4]-[\Sigma_5\text{ to }\Sigma_8]\}+\tfrac{1}{4}(-\sigma_{17}+3\sigma_{18})$$
$$+\tfrac{1}{3}\alpha+\mathrm{K}-(36),$$

$$45{\cdot}80753q_1=(1-\tfrac{1}{73})\{[\Delta_1\text{ to }\Delta_8]\}-\tfrac{1}{2}\delta_0-\tfrac{1}{3}\beta,$$

$$45{\cdot}72071p_2=(1-\tfrac{1}{73})\{[\Sigma_1\Sigma_2]-[\Sigma_3\text{ to }\Sigma_6]+[\Sigma_7\Sigma_8]\}$$
$$+\tfrac{1}{8}\{\sigma_{26}-3\sigma_8+5\sigma_9-7\sigma_{27}\}+\mathrm{K}+(36),$$

$$45{\cdot}72071q_2=(1-\tfrac{1}{73})\{[\Delta_1\text{ to }\Delta_4]-[\Delta_5\text{ to }\Delta_8]\}$$
$$+\tfrac{1}{4}\{-\delta_{17}-2\delta_0+3\delta_{18}\},$$

$$\alpha=45{\cdot}57452p_3=(1-\tfrac{1}{73})\{\Sigma_1-[\Sigma_2\text{ to }\Sigma_4]+[\Sigma_5\text{ to }\Sigma_7]-\Sigma_8\}$$
$$+\tfrac{1}{12}\{-\sigma_{29}+3\sigma_{17}-5\sigma_5+7\sigma_6-9\sigma_{18}+11\sigma_{30}\}+\mathrm{K}-(36),$$

$$\beta=45{\cdot}57452q_3=(1-\tfrac{1}{73})\{[\Delta_1\text{ to }\Delta_3]-[\Delta_4\Delta_5]+[\Delta_6\text{ to }\Delta_8]\}$$
$$+\tfrac{1}{6}\{\delta_{23}-2\delta_{11}-3\delta_0+4\delta_{12}-5\delta_{24}\},$$

$$46{\cdot}07016p_4 = (1-\tfrac{1}{72})\{\Sigma_9-\Sigma_{10}+\Sigma_{11}-\Sigma_{12}+\Sigma_{13}\}$$
$$+\tfrac{1}{8}\{\sigma_4-3\sigma_{13}+5\sigma_{22}-7\sigma_{31}\}+\mathrm{K}+(86),$$

$$45{\cdot}37025q_4 = (1-\tfrac{1}{72})\{[\Sigma_1\Sigma_2]-[\Sigma_3\Sigma_4]+[\Sigma_5\Sigma_6]-[\Sigma_7\Sigma_8]\}$$
$$+\tfrac{1}{8}\{-\delta_{26}+2\delta_{17}-3\delta_8-4\delta_0+5\delta_9-6\delta_{18}+7\delta_{27}\}.$$

The reciprocals of the numerical coefficients of the several p's and q's in the above equations will be the multipliers of the quantities represented by the right-hand portions of those equations, which will be obtained by suitable combinations of the five-day means. The multipliers are—

For p_1 and q_1 .... $\cdot 02183 = m$,

,, p_2 and q_2 .... $\cdot 02187 = m(1+\cdot 002)$,

,, p_3 and q_3 .... $\cdot 02194 = m(1+\cdot 005)$,

,, p_4 .... $\cdot 02173 = m(1-\cdot 005)$,

,, q_4 .... $02204 = m(1+\cdot 01)$.

A table calculated to give multiples of the quantity ·02183 will evidently suffice for the other coefficients, with the small corrections shown above.

To provide for cases (which are likely to be frequent) in which the series of terms is not truly recurrent, corrections similar to those required for the 24-term series must be applied. As before, representing by T_0 and T_{-1} respectively the mean values for the first day of the period under computation, and the last day of the preceding year, and by t_0 and t_{-1} respectively the mean values for the first day of the following year and the last day of the year under computation; the quantity that must be added to the first term of the series of 72 terms, corresponding to the initial midnight of the year, to make it equal to the first term of the next yearly series, will be $2c = \frac{1}{2}(t_0+t_{-1})-\frac{1}{2}(\mathrm{T}_0+\mathrm{T}_{-1})$, and it will be found that the corrections for the several p q coefficients will be as follows:—

r p_1 .... [illegible]$c \times$ For q_1 .... $31\frac{1}{3}c \times m$ $= \cdot 684c$,

, p_2 .... $c \times m(1+\cdot 002)$, ,, q_2 .... $17\ c \times m(1+\cdot 002) = \cdot 372c$,

, p_3 .... $c \times m(1+\cdot 005)$, ,, q_3 .... $11\ c \times m(1+\cdot 005) = \cdot 241c$,

, p_4 .... $c \times m(1-\cdot 005)$. ,, q_4 .... $8\ c \times m(1+\cdot 01)\ = \cdot 176c$.

For p_0, or the mean of the whole series, ... $= \frac{1}{72}c$.

A similar method of computation might be adopted in the case of a yearly period of 365 days, which could be transformed by interpolation into a series of 360 days, each term of which would correspond to $\frac{365}{360}$ days $= 1 + \frac{1}{72}$ days. But the irregularities of a series of daily quantities, in meteorological discussions, would be so great, even when dealing with the mean of many years, that no practical advantage would be obtained by employing such a series; the computations would be more troublesome, and the five-day means will be preferable.

10.

The annexed Forms are proposed as supplying methods of computation from the formulæ contained in the foregoing discussion, which shall involve the least practicable quantity of arithmetical operations.

Form 1 differs little from what is believed to be the ordinary method at present. It requires tables of multiples of the sines of 15°, 45°, 60° and 75°.

Form 2 is for the proposed new method of computation. It requires tables of multiples of the special multipliers applicable to the several orders of harmonic coefficients.

Form 3 requires no comment except to mention that the value of the angles which correspond to the hours of maximum μ will facilitate the graphic representation of the several components, and appear to characterise them better than the angles T.

Forms 4 and 5 call for no special remark. But they indicate the degree of divergence that exists between the approximate values of coefficients obtained from them, and those got by the more exact methods. The figures employed are the mean hourly temperatures for one year for the month of June, at Greenwich. If the mean values for a series of years had been dealt with, the results would not have differed one with another by so much as one-tenth of a degree Fahrenheit.

Form 6 shows the method of computation from 73 five-day mean values. The figures employed are mean temperatures at Königsberg for 24 years (taken from Bessel's paper, the translation of which is given in the 'Quarterly Weather Report' of the Meteorological Office, Part IV, 1870, page [29]), which were selected in order to test the agreement of the results with those given by Bessel, calculated by the method of least squares. The values of the coefficients by the two methods are virtually identical with the exception of p_3, in which I feel satisfied that some error has been committed in Bessel's calculation; to verify this, however, would involve a very tedious computation which I have not thought it worth while to undertake. It should be observed that Bessel's coefficients, being calculated with reference to a period commencing with the first of the five-day means, that is noon on the 3rd day of the year, have to be modified to adjust them to a period commencing with the initial midnight of the year. In order to render the form complete, as a type of the proposed system of computation, values for the first and last days of the year have been interpolated, and a small non-periodical correction has been assumed to be required; but these have no practical effect on the numerical results.

For this computation a table of multiples of the multiplier (*m*) is required, and to save trouble a table of multiples of $\frac{1}{72}$ has also been drawn out.

NOTE.—The Tables referred to will be printed by the Meteorological Office.

Form 1.—Computation of Harmonic Coefficients of the First Four Orders, in the Form $p \cos x$, $q \sin x$, according the method of Least Squares.

Hour.	a.		d.	−d 4 to 7.	+d 8 to 11.	θ.	θ_0 $\theta_1-\theta_2$	+25 +33	× 1 sin 45°	+ 25 + 23		S.	Σ.	Σ_0 1 −5	+34 +40	× 1 sin 60	+34 +35	p_2.
	0 to 11	12 to 23.																
0–12	58·8	72·5	−137	+146	+ 16	+25			+ 12	+ 48		1313	+ 34	2 −4	+23	sin 30	+12	
1–13	58·1	72·8	147	+127	+ 65	+45			p_3	+0·40		1309	+ 21			+ 12	+81	+0·68
2–14 3–15	57·8 57·5	73·0 72·9	152 154	+ 95 + 42	+ 97 +124	+40 +12	θ_3 $\theta_1+\theta_2$.	+40 +57	× 1 sin 45°	+ 40 + 40		1308 1304	+ 16 + 7	Σ_3 1 +5	+ 7 + 2	× 1 sin 30	+ 7 + 1	
4–16	57·3	71·9	146						+ 12	+ 80		1292	− 7	2 +4	+ 9	sin 60	+ 8	q_2.
5–17	57·7	70·4	127						q_3	+0·67		1281	− 19			+ 12	+16	+0·13
					p_1.				q_1.				σ.	ψ.				
6–18 7–19	59·2 62·3	68·7 66·5	− 95 − 42	d_6. d_5-d_7	−137 − 85	× 1 sin 15°	− 137 − 22	d_6 d_5+d_7	− 95 −169	× 1 sin 75°	− 95 − 163	1279 1288	2592 2597	− 9 + 6	ψ_0 $\frac{1}{2}(\psi_1-\psi_2)$	−13	− 9 − 7	p_4.
8–20	65·4	63·8	+ 16	4 − 8	−162	sin 30°	− 81	4 + 8	−130	sin 60°	− 113	1292	2600	+19		+12	−16	−0·13
9–21 10–22	68·1 69·8	61·6 60·1	+ 65 + 97	3 − 9 2 − 10	−219 −249	sin 45° sin 60°	− 155 − 216	3 + 9 2 + 10	− 89 − 55	sin 45° sin 30°	− 63 − 28	1297 1299	2601 2591		$\psi_1+\psi_3$ sin 60°	+25	+22	q_4.
11–23	71·2	58·8	+124	1 − 11	−271	sin 75°	− 262	1 + 11	− 23	sin 15°	− 6	1300	2581			+12		+0·18
$-a_0$.	58·8	2c				+12	− 873			+ 12	− 468	Sum	15562	Non-periodic correction $c = -·30$				
$+a_{23}$.	58·2	−0·6				p_1	−7·28			q_1	−3·90	Mean	64·84					

Coefficients	p_1	q_1.	p_2.	q_2.	p_3.	q_3.	p_4.	q_4.	Mean		p.	q_1.	q_2.	q_3.	q_4	Mean.
Uncorrected	−7·28	−3·90	+0·68	+0·13	+0·40	+0·67	−0·13	+0·18	64·84		$\frac{1}{12}c$	·63c	·81c	·20c	·14c	$\frac{1}{24}c$
Correction	−0·03	− ·19	− ·03	− ·09	− ·03	− ·06	−0·03	− ·04	− ·01		− ·03	− ·19	− ·09	− ·06	− ·04	− ·01
Corrected	−7·31	−4·09	+0·65	+0·04	+0·37	+0·61	−0·16	+0·14	64·83							

Form 2—Computation of Harmonic Coefficients of the First Four Orders in the Form $p \cos z$, $q \sin z$, according to the proposed method

Hour	a		d	p_1		p_3		q_1		q_3		s	Σ		$m+\Sigma_0$	+ 1	
	0 to 11	12 to 23													$-n$	+26	
0-12	58 8	72 5	−137	d_0	− 15							1313	+34		c	− 3	p_2
1-13	58 1	72 8	−147	d_1-d_{11}	− 21	−408	+	d_1+d_{11}	− 23			1309	+21	m +37	*	+94	+0 63
2-14	57 8	73 0	−15	d_2-d_{10}	− 249			d_2+d_{10}	− 55			1308	+16		$m+\Sigma_3$	+44	
3 15	5 5	72 9	−154	d_3-d_9	− 219			d_3+d_9	− 89	−167	+	1304	+ 7		$+n$	−26	
4-16	57 3	71 9	−146	d_4-d_8	− 162			d_4+d_8	−130			1292	− 7	n − 6	$5c$	−15	q_2
5-17	57 7	70 4	−1 7	d_5-d_7	− 85	−466	−	d_5+d_7	−169			1281	−19		*	+ 3	+0 02
				Sum	−1123	+ 58	α	d_6	− 95	−624	−		σ	ψ	ψ		
6-18	59 2	68 7	− 95	$+\frac{1}{2}\alpha$	+ 19	− 3	c	Sum	− 61	+ 97	β	1279	259	− 9	0+1−3	−22	
7-19	62 3	66 5	− 4	$\frac{1}{2}c$	− 4	+ 55	*	$-\frac{1}{2}\beta$	− 32	− 9	$3c$	1288	2597	+ 6	c	− 3	p_4
8-20	65 4	63 8	+ 16	*	−1108	+ 0 38		$10c$	− 30	+ 88	*	1292	2600	+19	*	−25	−0 16
9-21	68 1	61 6	+ 65	p_1	−7 29	p_3		*	−623	+0 61		1297	2601		1+3	+25	
10-22	69 8	60 1	+ 97					q_1	−4 10	q_3		1299	2591		$2c$	− 6	q_4
11 23	71 2	58 8	+124									1300	2581		*	+19	+0 14
Non periodic correction	$-a_0$		58 8	$2c$								Sum	15562	$\frac{1}{24}c$			
	$+a_{23}$		58 2	−0 6								Mean	64 84	−0 01	64 83	corrected mean	

The values of the coefficients are obtained by reference to tables prepared for the purpose from the quantities marked thus *

Form 3.—Computation of Harmonic Coefficients of the First Four Orders in the Form P sin (s+T).

	I.		II.		III.		IV.	
log p −log q	p− q−	2·86273 2·61278	p+ q+	1·79934 0·30103	p+ q+	1·57978 1·78533	p− q+	1·17609 1·14613
log tan t		10·24995		11·49831		9·79445		10·0299
t		60° 39′		72° 23′		31° 55′		46° 58′
log sin t		9·94033		9·97915		9·72325		9·86395
log p − log sin t		2·92240		1·82019		1·85653		1·31214
P		8·36		0·66		0·72		0·21
T		240° 39′		72° 23′		31° 55′		313° 2′
			$\frac{t}{2}$	36° 12′	$\frac{t}{3}$	10° 38′	$\frac{t}{4}$	11° 45′
μ		209° 21′		8° 48′		19° 22′		34° 15′

p.	q.	T.	μ First maximum.							
			I.		II.		III.		IV.	
+	+	t	90° −	t	45° −	$\frac{t}{2}$	30° −	$\frac{t}{3}$	22½° −	$\frac{t}{4}$
−	+	360° − t	90 +		45 +		30 +		22½ +	
−	−	180 + t	270 −		135 −		90 −		67½ −	
+	−	180 − t	270 +		135 +		90 +		67½ +	

Form 4.—Computation of Approximate Values of Harmonic Components of the First Four Orders

Hour	*a*		*d*	$-d_4$	$+d_8$	θ	$\frac{d}{2}$	$-C$ $-\frac{1}{8}\theta$	A $\frac{1}{2}d-\frac{1}{8}\theta$	S	Σ 4B	B		Components computed from values of the *p q* coefficients			
	0 to 11	12 to 23												A	B	C	D
0–12	58 8	72 5	−137	+146	+ 15	+25	−6 85	− 42	− 27	1 1	+34	+0 85		7 31	+0 65	+0 40	−0 16
1 13	58 1	72 8	−147	+127	+ 6	+4	− 35	− 75	−8 10	309	+ 1	+0 5		8 12	+0 58	+0 69	−0 07
2 14	57 8	3 0	−1 2	+ 95	+ 9	+4	−7 60	− 67	−8 2	1308	+16	+0 40		−8 38	+0 36	+0 61	+0 21
3 15	57 5	72 9	−1 4	+ 42	+1 4	+12	−7 70	− 20	− 90	304	+	+0 18		−8 06	+0 04	+0 17	
4–16	57 3	71 3	−146				−7 0	+ 42	−6 88	1292	− 7	−0 18		−7 19	−0 30		
5 17	57 7	0 4	−127				−6 35	+ 75	−5 60	128	−19	−0 48		5 84	−0 54		
											σ	↓ 8D	D				
6–18	59 2	68 7	− 55				−4 75	+ 67	−4 08	279	2592	− 9	−0 11	−4 09			
7 19	62 3	66 5	− 42				−2 10	+ 20	−1 90	1 88	259	+ 6	+0 08	−2 06			
8–20	65 4	63 8	+ 16				+0 80	− 4	+0 38	1292	2600	+19	+0 4	+0 11			
9 21	68 1	61 6	+ 65				+3 25	− 75	+2 50	1297	2601			+2 8			
10 22	69 8	60 1	+ 97				+4 85	− 67	+4 18	1299	2591			+4 28			
11 23	71 2	58 8	+124				+6 0	− 20	+6 0	1300	2581			+6 00			
										Sum	15562						
										Mean	62 84						

Form 6.—Computation of the Harmonic Coefficients of the First Four Orders for a Series of 73 Five-Day Means.

Part II (add to Part I)

Combined multiples of differences

Term	q_1		q_2		q_3		q_4	
0	−1	+0 41	−2	+0 82	−3	+1 23	−4	+1 64
8							−3	+13 23
9							+5	−18 45
11					−2	+9 08		
12					+4	−16 24		
17			−1	+6 09			+2	−12 18
18			+8	−16 59			−6	+33 18
23					+1	−4 50		
24					−5	+21 90		
26							−1	+4 02
27							+7	−7 86
Sum	+ −		+ −	6 91 16 59	+ −	31 21 20 74	+ −	52 07 58 49
+	2	+0 41	4	−9 68	6	+11 47	8	−6 42
		+0 21		−2 42		+2 08		−0 80

Combined multiples of sums of terms

Term	p_1		p_2		p_3		p_4	
4							+1	−4 57
5					−6	+17 25		
6					+7	−13 77		
8			−3	+6 03				
9			+5	−3 65				
13							−3	−10 29
17	−1	−7 69			+3	+23 07		
18	+3	+27 09			−9	−81 27		
22							+5	+79 15
26			+1	+21 10				
27			−7	−161 42				
29					−1	−24 08		
30					+11	+268 95		
31							−7	−179 97
Sum	+ −		+ −	27 13 165 07	+ −	309 27 119 14	+ −	79 15 194 83
+	4	+19 40	8	−137 94	12	+190 13	8	−115 68
(36)	−	+4 85 12 73	+	−17 24 12 73	−	+15 84 12 73	+	−14 46 12 73
$x-z$		−7 88 −0 17		−4 51 −0 17		+3 11 −0 17		−1 73 −0 17
		−8 05		−4 68		+2 94		−1 90

Pairs of terms

Sums σ	Differences δ
−7 67	−0 41
•	•
•	•
•	•
−4 57	•
−3 45	•
−1 97	•
•	•
−2 01	−4 41
−0 73	−3 69
•	•
•	−4 54
•	−4 06
+3 43	•
•	•
•	•
•	•
+7 69	−6 09
+9 03	−5 53
•	•
•	•
•	•
+15 83	•
•	−4 50
•	−4 58
•	•
+21 10	−4 02
+23 06	−3 98
•	•
+24 08	•
+24 45	•
+25 71	•
•	•
•	•
•	•
•	•

Five day means				Partial sums of terms		Combined half years sums		Sums and differences of half years sums of terms		
Term	1st half year	2nd half year	Term	1st half year	2nd half year	3 and 6 terms	4 and 8 terms	Sums Σ	Differences Δ	
0	−4 04	−3 63	72							
1	4 12	2 34	71							
2	3 72	3 09	70							
3	2 45	1 79	69	−1433	−1085		−1433 −1085	−2518	•	A
4	3 36	−1 21	68	−336	−121	−2030				
5	2 61	−0 84	67	−261	−084	−1290		−3320	−740	B
6	2 17	+0 20	66							
7	3 07	+0 84	65							
8	3 1	1 0	64	−847	+222	−847 +222		−625	−1069	C
9	2 21	1 48	63							
10	1 74	2 70	62							
11	1 26	3 8	61	−521	+766	−521 +766	−1701	+245	−1287	D
12	−0 72	4 34	60	−07	+334		+1238	−463	•	E
13	−1 12	4 55	59	−112	+455					
14	−0 11	5 47	58							
15	+0 05	5 50	57							
16	−0 25	5 75	56							
17	+0 80	6 89	55	+049	+2361	−135 +3150		+3015	−3285	F
18	+1 75	7 28	54							
19	3 02	8 48	53							
20	3 95	8 8	52							
21	4 96	9 46	51	+1368	+3404		+1417 +5765	+7182	•	G
22	5 37	10 46	50	+507	+1046	+2517				
23	6 12	10 62	49	+612	+1062	+5512		+8029	−2995	H
24	7 40	11 98	48							
25	7 73	12 4	47							
26	8 54	12 56	46	+2387	+3696	+2387 +3696		+6083	−1309	I
27	9 54	13 52	45							
28	9 75	13 76	44							
29	10 07	14 01	43	+2936	+4129	+936 +4129	+6968	+5065	−3193	K
30	10 33	14 12	42	+1033	+1412		+10299	+17267	•	L
31	11 50	14 21	41	+1150	+1421					
32	11 41	14 04	40							
33	11 02	13 27	39							
34	11 45	13 64	38							
35	+12 05	+13 63	37	+4593	+5458	+6776 +8291	+4593 +5458	+10051	•	M
36	+12 73							+15067	−1515	N
					+36	+1273				
					Sum + −	41655 4823				
					+73	36832				
					Mean	+5 045				

Mean, 31st Dec , 1st Jan	T_{-1} T_0	−4 00 −4 02	−8 02
Mean 31st Dec , 1st Jan	t_{-1} t_0	−3 74 −3 80	−7 54
$\frac{1}{2}(T_{-1}+T_0)$ $\frac{1}{2}(t_{-1}+t_0)$ $\frac{1}{2}(0+72)$	x y z	−4 01 −3 77 −3 84	
$x-z$		−0 17	
$y-x=2c$		+0 24	

Non periodic correction

Mean	p_1	$p_{2\text{ to }4}$	q_1	q_2	q_3	q_4
014c	029c	022c	684c	372c	241c	176c
00	00	00	+08	+04	+03	+02

Part I

Combined differences of half year's terms

Differences	q_1		q_2		q_3		q_4	
B	+	−7 40	+	−7 40	+	−7 40	+	−7 40
C	+	−10 69	+	−10 69	+	−10 69	+	−10 69
D	+	−12 87	+	−12 87	+	−12 87	−	+12 87
F	+	−32 85	+	−32 85	−	+32 85	−	+32 85
H	+	−29 95	−	+29 95	−	+29 95	+	−29 95
I	+	−13 09	−	+13 09	+	−13 09	+	−13 09
K	+	−11 93	−	+11 93	+	−11 93	−	+11 93
N	+	−15 15	−	+15 15	+	−15 15	−	+15 15
Sum	+ −		+ −	70 12 63 81	+ −	62 80 71 13	+ −	72 80 61 13
	−	133 93 1 86	+	6 31 0 09	−	8 33 0 12	+	11 67 0 15
Pt δ	− +	132 07 0 21	+ −	6 22 2 42	− +	8 21 2 08	+ −	11 52 0 80
$-\frac{\beta}{s}$	− +	131 86 2 04	+	3 80	−	6 13 β	+	10 72
	−	129 82						
Tab Corr		−2 84 +0 08		+0 08 +0 04		−0 13 +0 03		+0 23 +0 02
	q_1	−2 76	q_2	+0 12	q_3	−0 10	q_4	+0 25

Combined sums of half year's terms

Sums	p_1		p_2		p_3		p_4	
A	•		•		•		+	−25 16
B	+	−33 20	+	−33 20	+	−33 20	•	
C	+	−6 25	+	−6 25	−	+6 25	•	
D	+	+2 45	−	−2 45	−	−2 45	•	
E	•		•		•		−	+4 63
F	+	+30 15	−	−30 15	−	−30 15	•	
G	•		•		•		+	+71 82
H	−	−80 29	−	−80 29	+	+80 29	•	
I	−	−60 83	−	−60 83	+	+60 83	•	
K	−	−70 65	+	+70 65	+	+70 65	•	
L	•		•		•		−	−172 67
M	•		•		•		+	+100 51
N	−	−150 67	+	−150 67	−	−150 67	•	
Sum	+ −	32 60 401 89	+ −	221 32 213 17	+ −	218 02 216 47	+ −	176 96 197 85
$-\frac{\gamma}{3}$	−	369 29 5 13	+	8 15 11	+	1 55 02	−	20 89 29
Pt 2	− −	364 16 8 05	+ −	8 04 4 68	+ +	1 53 2 94	−	20 60 1 90
$+\frac{a}{s}$	− +	372 21 1 49		+3 36		+4 47 a		−22 50
	−	370 72						
Tab		−8 09		+0 07		+0 10		−0 49
Corr		0		0		0		0
	p_1	−8 09	p_2	+0 07	p_3	+0 10	p_4	−0 49

The quantities in the line marked 'Tab' are found in the tables of multiples of (m) from the figures in the line immediately above

Form 5.—Computation of Approximate Values of the Harmonic Coefficients of the First Three Orders.

Hours.	Observed.		Differences.	Sums.	p.			q.		
	0 to 11.	12 to 23.								
0........	58·8	72·5	−137	1313	$\frac{1}{2}d_0$	−6·9		$\frac{1}{2}d_6$	−4·8	
1........	58·1	72·8	•	•	$-p_3$	−0·4		$+q_3$	+0·7	
2........	57·8	73·0	−152	•		−7·3	p_1		−4·1	q_1
3........	57·5	72·9	•	1304	S_0	1313		S_3	1304	
4........	57·3	71·9	−146	•	$-S_6$	1279		$-S_9$	1297	
5........	57·7	70·4	•	•	$+4$	+34		$+4$	+ 7	
6........	59·2	68·7	− 95	1279		+0·8	p_2		+0·2	q_2
7........	62·3	66·5	•	•	d_0	−137		d_2	−152	
8........	65·4	63·8	+ 16	•	$-d_4$	+146		$-d_6$	+ 95	
9........	68·1	61·6	•	1297	$+d_8$	+ 16		$+d_{10}$	+ 97	
10........	69·8	60·1	+ 97	•	$+6$	+ 25		$+6$	+ 40	
11........	71·2	58·8	•	•		+0·4	p_3		+0·7	q_3

February 3, 1887.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Waves produced by a Single Impulse in Water of any Depth, or in a Dispersive Medium." By Sir W. THOMSON, Knt., LL.D., F.R.S. Received January 26, 1887.

For brevity and simplicity consider only the case of *two-dimensional motion.*

All that it is necessary to know of the medium is the relation between the wave-velocity and the wave-length of an endless procession of periodic waves. The result of our work will show us that the velocity of progress of a zero, or maximum, or minimum, in any part of a varying group of waves, is equal to the velocity of progress of periodic waves of wave-length equal to a certain length, which may be defined as the wave-length in the neighbourhood of the particular point looked to in the group (a length which will generally be intermediate between the distances from the point considered to its next-neighbour corresponding points on its two sides).

Let $f(m)$ denote the velocity of propagation corresponding to wave-length $2\pi/\lambda$. The Fourier-Cauchy-Poisson synthesis gives

$$u=\int_0^\infty dm \cos m[x-tf(m)] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

for the effect at place and time (x, t) of an infinitely intense disturbance at place and time $(0, 0)$. The principle of interference as set forth by Prof. Stokes and Lord Rayleigh in their theory of group-velocity and wave-velocity suggests the following treatment for this integral:—

When $x-tf(m)$ is very large, the parts of the integral (1) which lie on the two sides of a small range, $\mu-\alpha$ *to* $\mu+\alpha$, vanish by annulling interference; μ being a value, or the value, of m, which makes

$$\frac{d}{dm}\{m[x-tf(m)]\}=0\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

so that we have $$x = t\{f(\mu)+\mu f'(\mu)\} = yt, \tag{3}$$

where $$y = f(\mu)+\mu f'(\mu)\ * \tag{4}$$

and we have by Taylor's theorem for $m-\mu$ very small

$$m[x-tf(m)] = \mu[x-tf(\mu)]-\tfrac{1}{2}t[\mu f''(\mu)+2f'(\mu)](m-\mu)^2, \tag{5}$$

or, modifying by (3)

$$m[x-tf(m)] = t\{\mu^2 f'(\mu)+\tfrac{1}{2}[-\mu f''(\mu)-2f'(\mu)](m-\mu)^2\} \tag{6}$$

Put now $$m-\mu = \frac{\sigma\sqrt{2}}{t^{\frac{1}{2}}[-\mu f''(\mu)-2f'(\mu)]^{\frac{1}{2}}} \tag{7}$$

and using the result in (1) we find

$$u = \frac{\sqrt{2}\int_{-\infty}^{\infty} d\sigma \cos[t\mu^2 f'(\mu)+\sigma^2]}{t^{\frac{1}{2}}[-\mu f''(\mu)-2f'(\mu)]^{\frac{1}{2}}} \tag{8}$$

the limits of the integral being here $-\infty$ to ∞ because the denominator of (7) is so infinitely great that though $\pm a$ the arbitrary limits of $m-\mu$ are infinitely small a multiplied by it is infinitely great

Now we have $$\int_{-\infty}^{\infty} d\sigma \cos\sigma^2 = \int_{-\infty}^{\infty} d\sigma \sin\sigma^2 = \sqrt{(\tfrac{1}{2}\pi)} \tag{9}$$

Hence (8) becomes

$$u = \frac{\cos[t\mu^2 f'(\mu)]-\sin[t\mu^2 f'(\mu)]}{t^{\frac{1}{2}}[-\mu f''(\mu)-2f'(\mu)]^{\frac{1}{2}}} = \frac{\sqrt{2}\cos[t\mu^2 f'(\mu)+\tfrac{1}{4}\pi]}{t^{\frac{1}{2}}[-\mu f''(\mu)-2f'(\mu)]^{\frac{1}{2}}} \tag{10}$$

To prove the law of wave length and wave velocity for any point of the group remark that by (3)

$$t\mu^2 f'(\mu) = \mu[x-tf(\mu)]$$

and therefore the numerator of (10) is equal to $\sqrt{2}\cos\theta$ where

$$\theta = \mu[x-tf(\mu)]+\tfrac{1}{4}\pi \tag{10'}$$

and by (2) and (3) $$\frac{d}{d\mu}\{\mu[x-tf(\mu)]\} = 0$$

by which we see that

$$d\theta/dx = \mu, \quad \text{and} \quad d\theta/dt = -\mu f(\mu) \tag{10''}$$

which proves the proposition

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* This is the group velocity according to Lord Rayleigh's generalisation of Prof. Stokes's original result

Example (1).—As a first example take deep-sea waves; we have

$$f(m) = \sqrt{\frac{g}{m}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

which reduces (4), (3), and (10) to

$$y = \tfrac{1}{2}\sqrt{\frac{g}{\mu}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

and

$$x = \tfrac{1}{2}\sqrt{\frac{g}{\mu}}.\,t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$u = \frac{1}{g\sqrt{2}}\frac{t}{x^{\frac{1}{2}}}\left(\cos\frac{gt^2}{4x} + \sin\frac{gt^2}{4x}\right) = \frac{t}{gx}\cos\left(\frac{gt^2}{4x} - \frac{\pi}{4}\right), \quad . \quad (14)$$

which is Cauchy and Poisson's result for places where x is very great in comparison with the wave-length $2\pi/\mu$, that is to say, for place and time such that $gt^2/4x$ is very large.

Example (2).—Waves in water of depth D:

$$f(m) = \sqrt{\left\{\frac{g}{m}\frac{1-\epsilon^{-2m\mathrm{D}}}{1+\epsilon^{-2m\mathrm{D}}}\right\}}. \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Example (3).—Light in a dispersive medium.

Example (4).—Capillary gravitational waves:

$$f(m) = \sqrt{\left(\frac{g}{m} + \mathrm{T}m\right)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Example (5).—Capillary waves:

$$f(m) = \sqrt{(\mathrm{T}m)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

Example (6).—Waves of flexure running along a uniform elastic rod:

$$f(m) = m\sqrt{\frac{\mathrm{B}}{w}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

where B denotes the flexural rigidity, and w the mass per unit of length.

These last three examples have been taken by Lord Rayleigh as applications of his generalisation of the theory of group-velocity; and he has pointed out in his "Standing Waves in Running Water" (London Mathematical Society, December 13, 1883) the important peculiarity of Example (4) in respect to the critical wave-length which gives minimum wave-velocity, and therefore group-velocity equal to wave-velocity. The working out of our present problem for this case, or any case in which there are either minimums or maximums, or both maximums and minimums, of wave-velocity, is particularly interesting, but time does not permit its being included in the present communication.

For Examples (5) and (6) the denominator of (10) is imaginary; and the proper modification, from (7) forwards, gives for these and such cases, instead of (14), the following :—

$$u = \frac{\cos\left[t\mu^2 f'(\mu)\right] + \sin\left[t\mu^2 f'(\mu)\right]}{t^{\frac{1}{2}}\left[\mu f''(\mu) + 2f'(\mu)\right]^{\frac{1}{2}}} \quad . \quad . \quad . \quad . \quad . \quad (19)$$

The result is easily written down for each of the two last cases [Examples (5) and (6)].

II. "On the Formation of Coreless Vortices by the Motion of a Solid through an inviscid incompressible Fluid." By Sir W. THOMSON, Knt., LL.D., F.R.S. Received February 1, 1887.

Take the simplest case: let the moving solid be a globe, and let the fluid be of infinite extent in all directions. Let its pressure be of any given value, P, at infinite distances from the globe, and let the globe be kept moving with a given constant velocity, V.

If the fluid keeps everywhere in contact with the globe, its velocity relatively to the globe at the equator (which is the place of greatest relative velocity) is $\frac{3}{2}$V. Hence, unless $P > \frac{9}{8}V^2$,* the fluid will not remain in contact with the globe.

Suppose, in the first place, P to have been $> \frac{9}{8}V^2$, and to be suddenly reduced to some constant value $< \frac{9}{8}V^2$. The fluid will be thrown off the globe at a belt of a certain breadth, and a violently disturbed motion will ensue. To describe it, it will be convenient to speak of velocities and motions *relative to the globe*. The fluid must, as indicated by the arrow-heads in fig. 1, flow partly backwards and partly forwards, at the place, I, where it impinges on the globe, after having shot off at a tangent at A. The back-flow along the belt that had been bared must bring to E some fluid; and the free surface of this fluid must collide with the surface of the fluid leaving the globe at A. It might be supposed that the result of this collision would be a "vortex sheet," which in virtue of its instability, would get drawn out and mixed up indefinitely, and be carried away by the fluid farther and farther from the globe. A definite amount of kinetic energy would be *practically annulled* in a manner which I hope to explain in an early communication to the Royal Society of Edinburgh.

But it is impossible, either in our ideal inviscid incompressible

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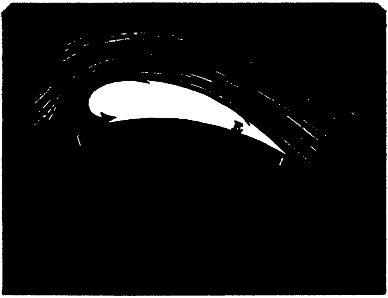
* The density of the fluid is taken as unity.

Fig. 1.

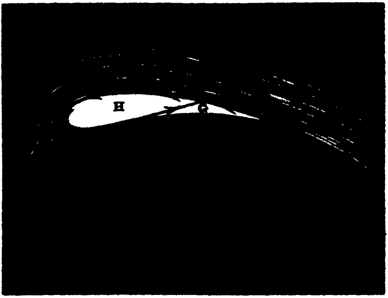


fluid, or in a real fluid such as water or air, to form a vortex sheet, that is to say an interface of finite slip by any natural action. What happens in the case at present under consideration, and in every real and imaginable case of two portions of liquid meeting one another, as for instance a drop of rain falling directly or obliquely on a horizontal surface of still water, is that continuity and the law of continuous fluid motion become established at the instant of first contact between two points, or between two lines in a class of cases of ideal symmetry to which our present subject belongs.

An inevitable result of the separation of the liquid from the solid, whether our supposed globe or any other figure perfectly symmetrical round an axis and moving exactly in the line of the axis, is that two circles of the freed liquid surface come into contact and initiate in an instant the enclosure of two rings of vacuum (G and H in fig. 2, which, however, may be enormously far from like the true configuration).

The "circulation" (line-integral of tangential component velocity round any endless curve encircling the ring, as a ring on a ring, or one of two rings linked together) is determinate for each of these vacuum-rings, and remains constant for ever after: unless it divides

FIG 2



itself into two or more, or the two first formed unite into one, against which accidents there is no security.

It is conceivably possible* that a coreless ring vortex, with irrotational circulation round its hollow, shall be left oscillating in the neighbourhood of the equator of the globe; *provided* $(\frac{1}{2}V^2-P)/P$ *be not too great*. If the material of the globe be viscously elastic, the vortex settles to a steady position round the equator, in a shape perfectly symmetrical on the two sides of the equatoreal plane; and the whole motion goes on steadily henceforth for ever.

If $(\frac{1}{2}V^2-P)/P$ exceed a certain limit, I suppose coreless vortices will be successively formed and shed off behind the globe in its motion through the fluid, incessantly.

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* If this conceivable possibility be impossible for a globe, it is certainly possible for some classes of prolate figures of revolution.

III "On *Proterosaurus Speneri* (von Meyer)" By H. G **Seeley**, F R S, Professor of Geography in King's College, London. Received February 3, 1887

(Abstract)

The author gives an account of the scientific history of Proterosaurus, and states the interpretations of its structure given by Cuvier, von Meyer, Sir R Owen, and Professor Huxley

In Part II he describes the type specimen in the Museum of the Royal College of Surgeons In the skull characters are given of the cerebral cavity, the supra-occipital, parietal, frontal, pre frontal, nasal, and premaxillary bones A restoration is made of the skull and the teeth are shown to be anchylosed to the jaw On the palate the vomer, palatine, and pterygoid bones are described and shown to have all been armed with minute teeth The pterygoid bone was strongly united to the quadrate bone The lower jaw and hyoid bones are also described

In the vertebral column a description is given of the second to the seventh cervical vertebræ, of sixteen dorsal vertebræ, two sacral vertebræ, and twenty three caudal vertebræ

The femur tibia and fibula and foot are also described The skin is found to have been defended with a bony armour

In Part III comparison is made between the type and other specimens which have been referred to it with the result that some are regarded as indicating different species while others indicate different genera

In Part IV a comparison is made to show the resemblances of Proterosaurus with other reptiles in the several regions of the skeleton, with the result that the Proterosauria is regarded as a distinct division of the Reptilia, showing resemblances to many of the highly specialised orders and to some low types

Presents, February 3 1887

Transactions

Baltimore —Johns Hopkins University Studies in Historical and Political Science Fifth Series Nos 1–2 8vo *Baltimore* 1887 The University

Christiania —University Om Humanisten og Satirikeren Johan Lauremberg 8vo *Christiania* 1884, Antinoos, eine Kunst archaeologische Untersuchung 8vo *Christiania* 1884, Lakis Kratere og Lavastrømme 4to *Kristiania* 1886, Norges Vaextrige Bind I. 4to *Christiania* 1885 The University

Transactions (*continued*)

Eastbourne —Natural History Society Transactions Vol I Parts 9–10 8vo *Eastbourne* 1885–86 The Society

London —Sanitary Institute Transactions Vol VII 8vo *London* 1886 The Institute

Paris —École Normale Supérieure Annales Année 1887 No 1 4to *Paris* The School

Pesth —Magyar Tudományos Académia Almanach 1885 Archæologiai Értesitő 1884, Kotet IV 1885 Kotet V Szám 1–2 Bulletin 1884–85 1 3 Emlékbeszédek Kotet II Szám 8–10 Kotet III Szám 1 2 Értekezesek (Nemzetgazdaság) Kotet II Szam 6 Értekezések (Nyelvtudományi) Kotet XI Szám 11 12 Kotet XII Szám 1 5 Értekezesek (Mathematikai) Kotet XI Szám 1 9 Értekezések (Tarsadalmi) Kotet VII Szám 8–9 Értekezesek (Természettudományi) Kotet XIV Szam 1 8 Értekezések (Torténettudományi) Kotet XI Szám 7 10 Kotet XII Szám 1–2 4 Évkónyv 1884 Vol 2 Évkonyvei Kotet XVII No 2 Legtuneti Észleletek Kotet II Mathematikai és Termész Értesitő Kotet III Fuzet 1 5 Mathematikai és Termész Kozlemények Kotet XVIII Kotet XIX Mathematische und Naturwissenschaftliche Berichte Bd II Nyelvtudomány Kozlemények Kotet XVIII Fuzet 2 3 Kotet XIX Fuzet 1 Repertorium Kotet I Resz 1 Ungarische Revue 1885 Heft 1 7 Értesitője Kotet XVIII Szám 3–7 Kotet XIX Szám 1 2 Konig Egyenletek 8vo *Budapest* 1885 Alsó Magyar bányamüvelesének torténete Kotet I 8vo *Budapest* 1884 Régi Magyar Konyvtar Kotet II 8vo *Budapest* 1885 Bartfai Konyvtár 8vo *Budapest* 1885 Vázlatók az Akadémia 1831–81 8vo *Budapest* 1881 Magyarország II József Korában II 8vo *Budapest* 1884 Monumenta Comitiorum Transsylvaniæ Kotet X 8vo *Budapest* 1884 Monumenta Hungariæ Kotet I 8vo *Budapest* 1885 Nyelvemléktár Kotet XI Kotet XII 8vo *Budapest* 1884 Codex Diplomaticus Kotet IV 8vo *Budapest* 1884 Bethlen és a svéd Diplomatia. 8vo *Budapest* 1882 Æmilius Papinianus 8vo *Budapest* 1884 A Keresztény ség fondamentomáról 12mo Epistolæ Sancti Pauli 12mo A Keszthelyi Sirmezok 4to *Budapest* 1884 The Academy

Trondhjem —K N Videnskabers Selskab Skrifter 1885 8vo *Trondhjem* 1886 The Society

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Observations and Reports

Christiania —Fjerde Beretning om Bygdø Kongsgaard med Tillæg 4to *Christiania* 1886 The University Christiania

Observations, &c. (*continued*).

London:—Kew Gardens. Bulletin of Miscellaneous Information. No. 1. 8vo. *London* 1887. The Director.

Meteorological Office. Weekly Weather Report. Nos. 42–52. 4to. Quarterly Summary of Weekly Report. Third Quarter, 1886. 4to.; Monthly Weather Report. July–August, 1886. 4to. The Office.

Melbourne:—Department of Mines. Reports of the Mining Registrars, quarter ended September, 1886. Folio. *Melbourne* 1886. The Department.

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Montreal:—Geological and Natural History Survey of Canada. Annual Report. 1885 With Maps. 8vo. *Montreal* 1886. The Survey.

Pesth:—Konig. Ung Geologische Anstalt. Die Kon. Ung. Geologische Anstalt und deren Ausstellungs-Objecte, &c. 8vo. *Budapest* 1885; Foldtani Intézet es ennek Kiállítási Tárgyai. 8vo. *Budapest* 1885; Special-Katalog der VI-ten Gruppe 8vo. *Budapest* 1885; Der Goldbergbau Siebenburgens. 8vo *Budapest* 1885. With six other pamphlets in 8vo. The Institution.

February 10, 1887.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Contributions to the Metallurgy of Bismuth." By EDWARD MATTHEY, F.S.A., F.C.S., Assoc. Roy. Sch. Mines. Communicated by JOHN PERCY, M.D., F.R.S., Pres. Iron and Steel Inst. Received August 18, 1886.

§ 1. *Bismuth: its Separation from Gold, and its Refining Action upon same during the Process of Separation.*—In bringing the above subject under notice, it is necessary to allude to some of the facts distinguishing this very interesting metal.

Bismuth, in some of its important characteristics and reactions, resembles lead. And one of the chief points of resemblance between these metals is their ready oxidation, and their absorption by bone-ashes or wood-ashes whilst so oxydised. I refer of course, to the process of cupellation.

This ancient and serviceable process, still employed universally for the separation of gold and silver from lead, is equally applicable to bismuth, if associated with these precious metals; and, like lead, bismuth may be readily employed as a vehicle or means of collecting gold and silver from their ores in reduction processes; but its comparative cost accounts for its non-employment in this respect.

Commercially speaking, bismuth differs from lead in its greater value, lead being worth at present £13 to £14 per ton,[*] whilst bismuth realises between £700 and £800 per ton; this high value being due to its greater rarity and to its limited and special uses.

As is well known, bismuth ores are frequently auriferous; and one of the points which it is my desire to bring under notice is the effectual separation of the gold from bismuth by a rapid and efficacious process.

Of course, nothing could be easier than to separate these two metals by the ordinary process of cupellation. The gold, by these means, is at once rendered available, but with the drawback that not only is there a very considerable loss of bismuth by volatilisation during the cupellation, but the subsequent recovery of the metal, which in the state of oxide has been absorbed by the cupel, is rendered necessary, involving a tedious and troublesome smelting operation, the employment of expensive fluxes, and a further considerable loss of metal

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[*] June, 1886.

Bearing in mind the close resemblance of bismuth to lead in its behaviour in the cupellation process, I directed my attention to its separation from gold by means of the addition of a small proportion of zinc—a method known as the Parkes process as employed for the separation of silver from lead And this I found successful, the natural separation of these two metals during the process of cooling proving to be similar in both cases

The operation as carried out by me is as follows —

The Bismuth holding the gold is melted at the ordinary temperature about two per cent of melted zinc is then added, and the whole brought to a dull red heat The alloy is then well stirred, and the temperature gradually lowered When at a black heat the slight crust formed on the surface is skimmed off and the metal again treated with a further quantity of zinc at the higher temperature *The whole of the gold* will be found in these skimmings, and the bismuth will be thus freed from it

The skimmings, consisting of bismuth, gold and zinc, and zinc oxide, I now treat by a process which quickly renders the gold available, and at the same time has the effect of *refining* the gold from all impurities excepting silver during the actual process of extraction

This small proportion of bismuth litharge and its charge of gold is fused in a clay crucible with a little borax, and allowed to cool down in the crucible, or it is poured into a mould with the bismuth litharge, which being perfectly liquid, allows the metallic gold to separate by its own gravity, and during its fusion absorbs any base metals associated with it as oxides *The bismuth litharge, in fact, acts as a refining agent to the gold*, which, when cold, is detached from it This bismuth slag is broken up, re fused with a little metallic bismuth, and is so freed from the last trace of gold which is collected by the bismuth and subsequently extracted The bismuth litharge so freed from gold is then reduced by fusion with carbon to its metallic state

The quantity of bismuth litharge holding the gold is exceedingly small in proportion to the bulk of metal originally treated, as the figures hereinafter given will show, but, by this process the bismuth is at once freed from its gold contents with little time, labour, or expense

I have continuously carried out this method of treatment with the most satisfactory results It will only be necessary to take the figures of one operation as an illustration

A quantity of 9483 lbs of bismuth, holding about one per cent of impurity, and 12 5 ounces of gold per ton (equal to 53 5 ounces in the bulk), was so treated, and of this nearly 9000 lbs was immediately rendered available for commercial use, the skimmings, which amounted to 658 lbs (7 30 per cent of the bulk), *containing the whole of the gold*

These skimmings I oxidised by means of nitric acid, thus obtaining the greater proportion of the bismuth and what little copper there was in solution, from which the bismuth was precipitated by the ordinary method, care being taken to saturate the nitric acid by extracting the greater portion of the bismuth as nitrate, so as to leave a portion of the bismuth as oxide with the gold in order to refine it from the impurities existing as oxides when fused with it This residue, collected and dried, was, when dried, fused in clay crucibles, with a small quantity of borax, yielding the full amount of gold shown by assay

As before stated, in these fusions the metallic gold separates from the bismuth litharges, and descends to the bottom of the crucible by its own gravity The liquid and supernatant bismuth litharge floats upon it and breaks away readily when cold, the gold so obtained being associated only with silver, both metals being in fact *refined by the action of the bismuth litharge*

§ 2 *Separation of Bismuth from Lead*—The difficulty surrounding the treatment of bismuth associated with other metals by any rapid or comprehensive process is well known to the metallurgical chemist I believe I am correct in stating that hitherto the only process employed for the refining of bismuth on the Continent—notably in Saxony, the chief continental source of this metal—has been that of chlorination and subsequent precipitation, a process tedious in itself and involving much plant and labour in comparison with the quantities of metal operated upon

Rapidity of production with a minimum margin of loss, in order to free the metal from its impurities and render it marketable as quickly as possible, being a great desideratum, induced me to turn my attention to its refining by dry processes In carrying this out I have found present most of the metals which are easily seized by and become associated with the bismuth itself during the process of reduction from its ores,* such as antimony, arsenic, tellurium, lead, copper, &c., &c , all of which I have successively and successfully dealt with

It is not my intention in this paper to describe the processes adopted for the elimination of these several metals, but to confine myself to the separation of lead, the presence of which especially presented at first great difficulties.

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* See Table of Analyses herewith

As stated above, I have found that I can separate one by one the metals mentioned above, all of which have been associated with crude bismuth which has come under my notice. In this, success though gradual, has been complete: but I was still confronted by the fact that the lead alloy was retained by the bismuth with a most characteristic persistency which seemed to defy all efforts of separation excepting by tedious wet or acid processes.

The amount of lead existing in the bismuth I operated upon, after freeing it by dry processes from its other impurities, varied from 2 to 10 per cent.

Bearing in mind the respective fusing points of lead and bismuth, it occurred to me that, as alloys of bismuth and lead fuse at a temperature considerably lower than that of bismuth itself, separation would possibly take place between the two metals at a certain point of cooling; I therefore made the following experiment:—

Taking a quantity of bismuth (about 10 cwt.), holding 11·5 per cent. of lead, and fusing same, I allowed the metal to cool until the major part of it had crystallised, then removing the fluid portion.

The residue showed by assay only 6·35 per cent. of lead, pointing at once to the partial separation I had hoped for.

These crystals again similarly treated showed only 3·75 per cent. of lead.

The operation repeated gave crystals with only 2 per cent. of lead, and a fourth crystallisation brought this down to below 0·5 per cent.

As a matter of possible interest, I subjoin the progressive results during the crystallising operations of several lots up to the point of bulking, and of finally separating every trace of lead:—

Bismuth holding 14·6 *per cent. Lead.*

1st	crystallisation	gave crystals	holding	9·8	per cent.	of lead.
2nd	,,	,,	,,	5·1	,,	,,
3rd	,,	,,	,,	3·8	,,	,,
4th	,,	,,	,,	2·5	,,	,,
5th	,,	,,	,,	0·4	,,	,,

Bismuth holding 12 *per cent. Lead.*

1st	crystallisation	gave crystals	holding	6·2	per cent.	of lead.
2nd	,,	,,	,,	4·2	,,	,,
3rd	,,	,,	,,	1·4	,,	,,
4th	,,	,,	,,	0·4	,,	,,

Bismuth holding 7·6 per cent. Lead.

1st crystallisation gave crystals holding 4·8 per cent. of lead.
2nd ,, ,, ,, 3·8 ,, ,,
3rd ,, ,, ,, 0·8 ,, ,,
4th ,, ,, ,, 0·4 ,, ,,

Bismuth holding 11 per cent. Lead.

1st crystallisation gave crystals holding 5·5 per cent. of lead.
2nd ,, ,, ,, 2·5 ,, ,,
3rd ,, ,, ,, 1·0 ,, ,,

Bismuth holding 5·6 per cent. Lead

1st crystallisation gave crystals holding 2·0 per cent. of lead
2nd ,, ,, ,, 0·7 ,, ,,
3rd ,, ,, under 0·5 ,, ,,

Bismuth holding 5 3 per cent. Lead.

1st crystallisation gave crystals holding 1·8 per cent. of lead.
2nd ,, ,, ,, 0·6 ,, ,,
3rd ,, ,, under 0·5 ,, ,,

Having attained this point, I worked upon several large quantities of metal—with practically the same results—finally succeeding by a continuation of the process in eliminating every trace of lead.

By the above it will be seen that the process becomes an exceedingly simple one, large quantities being treated at one time, involving little or no loss, and occupying hours, instead of possibly weeks.

To illustrate the facilities of the separation of lead and bismuth alloys, I give the following figures from metal holding originally five per cent. of lead.

10,675 lbs. produced, in the course of six to seven crystallisations, 9306 lbs. of available bismuth, the residue 1188 lbs. holding 40 per cent. of lead, so that from a quantity of nearly 5 tons of bismuth and lead alloy only about half a ton remained, holding practically the whole of the lead; the bulk of the bismuth separated by simple crystallisation holding traces only of lead, which, if necessary, could be readily eliminated by further crystallisation. From these facts it is apparent that the separation of these two metals can be effected by turning to account their *relative fusing points*.

Recapitulation of foregoing Experiment

10,675 lbs leady bismuth, holding five per cent. lead, yielded 9806 lbs of good commercial bismuth by the crystallization process, or within six per cent of the total contents of pure bismuth

Leaving for subsequent treatment—

Of alloy, holding 40 per cent of lead, 1188 lbs, which is equal to 11 13 per cent of the whole weight of metal treated

Average Analysis of the Bismuth Ores worked upon

Bismuth	44 57
Lead	2 35
Antimony	0 64
Arsenic	1 26
Molybdenum	5 02
Tellurium	0 17
Iron	5 25
Manganese	0 05
Copper	0 24
Tungstic acid	2 45
Alumina	0 18
Magnesia	0 09
Lime	0 81
Carbonic acid	1 47
Sulphur	3 77
Insoluble earthy matter, chiefly silica	23 12
Water	3 37
Oxygen in combination and loss	5 19
	100 00

II. "An Inquiry into the Cause and Extent of a special Colour-Relation between certain exposed Lepidopterous Pupæ and the Surfaces which immediately surround them" By EDWARD B POULTON, M A, of Jesus and Keble Colleges, Oxford, Lecturer in Zoology and Comparative Anatomy at St Mary's Hospital, Paddington Communicated by Professor E. RAY LANKESTER, F R S Received February 10, 1887

(Abstract)

Historical—Mr T W Wood first called attention to the colour-relation in pupæ ('Entom Soc Proc,' 1867, p xcix), adducing instances of *Pieris brassicæ*, *P rapæ*, *Vanessa polychloros*, and (erroneously) *Papilio machaon* He even suggested that gilded surfaces might probably be found to produce gilded pupæ, but the experiment has never been made until the present investigation His observations were disputed by many entomologists but were confirmed by Mr A G Butler and Professor Meldola ('Zool Soc Proc,' 1878) Finally, Mrs Barber ('Entom Soc Trans,' 1874, p 519) obtained striking results with the pupæ of *Papilio nireus* (South Africa) which were confirmed by Mr Roland Trimen, who experimented upon *Papilio demoleus* Still later Fritz Muller ('Kosmos' vol 12, p 448) argues that the dimorphic pupæ of *Papilio polydamus* do not possess the colour-relation It was generally assumed that all the above instances of the colour relation were to be explained by supposing the skin of the freshly formed pupa to be "photographically sensitive, but the explanation was never tested by any system of transference to other colours, and Professor Meldola pointed out in 1874 that there was no real analogy with photography Furthermore the explanation failed to account for the colour of pupæ which threw off the larval skin on a dark night I therefore thought that the problem would probably prove to be essentially physiological, and that the reflected light would be found to act on the larva at some time before pupation and not upon the pupa itself and it seemed probable that the sensitive area might be defined by experiment The investigation was conducted in the summer and autumn of 1886

I *Experiments upon Vanessa Io* —Material was kindly supplied by Mr E D Y Pode, of Slade, Ivybridge Six mature larvæ were placed in a glass cylinder surrounded by yellowish green tissue paper, and all suspended themselves from the paper roof Five changed into the rarer yellowish green form of pupa, and the sixth immediately after the skin had been thrown off and while still moist and with the shape unformed, was transferred to a black surface in darkness but the pupal colours deepened into a yellowish green tint exactly like that of the other five pupæ

This experiment, so far as it went, confirmed my anticipation of larval as opposed to pupal susceptibility, and added another striking instance of pupal colour relation Mr W H Harwood, of Colchester, also informed me that he had found the same variety of this species on the under side of nettle leaves, but not the dark form which occurs commonly on walls, stones, &c Hence the protective value of the colour-relation is well seen, the species having varieties suitable for vegetal and mineral surroundings, and adjustable by the stimulus supplied by the colours of the environment

II *Experiments upon Vanessa urticæ* —This species was investigated in great detail, over 700 individuals being employed in the experiments Material was in part supplied by Mr. Pode, but chiefly found near Oxford. The first necessity was the construction of a standard list of colours with which to compare the pupæ which had been the subjects of experiment. The pupæ are very variable, and in many of the experiments the colour influence was only allowed to act during a small part of the time during which the larvæ are sensitive. Hence the careful record of minute differences was absolutely necessary, and the standard list was made as detailed as possible. The list was as follows:—

The degree of colour represented by—

(1.) Very dark, from the large amount of cuticular pigment; no gilding or the merest trace.

(2.) Dark normal form, but not so black as (1) and sometimes more gilding but very little.

(3.) Light normal form, sometimes with a fair amount of gilding, often with a predominant pinkish tint. This degree was afterwards subdivided into dark (3), (3), and light (3), and even further in certain experiments.

(4.) Very light variety, often extremely golden; sometimes light pink.

(5.) The lightest variety; often completely covered with the gilded appearance.

In the experiments summarised below, the individuals belonging to different companies were always separated, except in the larvæ subjected to green surroundings, so that the errors from varying hereditary tendencies were reduced to a minimum, for the larvæ of each company are hatched from the eggs laid by a single butterfly.

1. *The Results of Different Colours.*—*Orange* surroundings produced no effect, as far as the experiment went, for the few pupæ were all (3) and therefore showed no relation to the colour of the environment. After the experiment upon the allied *V. Io* I tried the effects of *green* upon a large number of individuals, but the resulting pupæ were on the whole rather darker than usual, probably because of the amount of shade produced by the tissue-paper. This conclusion suggested the use of *black* surroundings, and at once an immense effect was witnessed. These effects in turn suggested the use of *white* surroundings (white paper and white opal glass) and here also a powerful influence was exerted, the pupæ being often brilliantly golden in appearance. But it was clear that the very dark varieties were much better protected against the black surfaces than the lustrous golden pupæ against the white surfaces, and this consideration suggested the use of a material with which the golden appearance harmonised most perfectly, *i.e.*, metallic gold. Boxes and cylinders lined with gilt paper and turned towards a strong light produced the most extremely gilded varieties in a large proportion of the pupæ, and the metallic appearance was yellower and more truly golden than in the more silvery forms

produced by the use of white surroundings. The totals obtained by the use of these different surroundings were as follows (omitting the orange).

Degrees of colour.	(1)	(2)	Dark (3)	(3.)	Light (3)	(4)	(5.)	
Green surroundings	2	8	..	25	..	1	3	= 39
Black "	11	29	27	22	14	2	..	= 105
White "	..	7	21	37	44	25	11	= 145
Gilt "	..	1	2	7	16	27	14	= 67
								356

2. *The Colours of Wild Pupæ.*—It is impossible to realise the extremely remarkable results of the gilt and white surroundings without taking into account the fact that a (4) or a (5) is very rarely seen in the field, except when the pupa is diseased. Out of fifteen wild pupæ found August 31 on a grey stone wall, the lightest pupæ were (3), while there were four of the degree represented by (1), and there was only the minutest spot of gold to be seen after careful examination on two of the pupæ, and none on any of the others.

3. *The Effects of Mutual Proximity.*—Inasmuch as the above figures show that the larvæ are sensitive to dark surfaces and the larvæ themselves are almost black, it appeared probable that they would be mutually influenced when a large number pupated in close proximity. This was incidentally shown to be the case in several of the experiments, of which the most striking was as follows. Four larvæ were placed each in a separate cylinder while twelve were placed together in another similar cylinder, all having the same conditions of light and each cylinder lined and roofed with an equal amount of white paper and each standing upon an opal glass floor. Of the twelve larvæ ten pupated in close proximity upon the roof and sides, and were all light (3), while the remaining two pupated on the floor and were both (4). Of the four pupæ in separate cylinders two were (4) and two were (5). In consequence of these and other equally convincing results the exact position of the pupæ has to be taken into account in estimating the influences which have been at work, and in all the most careful experiments only one or two larvæ were placed in each coloured case.

4. *The Effects of Illumination.*—One experiment was directed towards the comparison of the influence of a black surface in strong light and the same surface in darkness, and the results show clearly

that the pupæ are on the whole darker in the latter circumstances, although dark under both conditions

In another experiment some larvæ were suspended in a strong direct light without any coloured background sufficiently near to affect them, and as far as the experiment went it indicated that there was an influence in the direction of the lighter varieties but in this case the numbers employed were too small to be convincing

5 *The Time during which the Larvæ are Sensitive*—The whole period preparatory to pupation, intervening between the cessation of feeding and pupation itself, may be divided into three stages (i) in which the larva descends from the food plant and wanders about in search of some (generally mineral) surface upon which to pupate, (ii) in which it rests motionless, usually in a curved position, upon the surface selected, (iii) in which it hangs head downwards suspended by its posterior claspers from a boss of silk spun at the close of the last stage The duration of Stage (i) depends upon the varying proximity of suitable surfaces, and it was always greatly curtailed in confinement, because such surfaces were close at hand If the larva is sensitive during this stage the influences cannot generally contribute towards the result because the larva is wandering over surfaces of various colours It is also very improbable that the larva can be sensitive after the first few hours or at any rate the first half of Stage (iii), because rapid changes are taking place under the larval skin, and it is even likely that processes are already on their way towards completion which will result in the formation of pigment or other substances which will many hours later deepen into the effective causes of pupal colour The length of Stage (iii) did not vary very much in different larvæ and in the shortest case observed the length was about 14 hours, while 20 hours was an unusually long period, but the majority of larvæ passed about 17 or 18 hours in this stage Stage (ii) was more variable but about 15 hours was a common length, while 36 hours is a fair estimate of the length of the whole preparatory period In the majority of cases a larva is probably sensitive to the colour of surrounding surfaces for about 20 hours preceding the last 12 hours of the whole preparatory period Thus the length is amply sufficient to include many hours of daylight during which the surrounding surfaces are illuminated If a larva be disturbed when Stage (ii) is far advanced the whole period begins again, and all the three stages are again passed through, but they are all abbreviated, including Stage (iii) which had not previously commenced Many experiments indicated that darkness may increase the length of the stages, but my observations were not specially directed towards the settlement of this question, which only occurred to me when the notes were tabulated Therefore I propose to specially investigate this point in the next season Such prolongation,

if corroborated, may be physiologically connected with pigment formation, or it may merely give the larva an additional opportunity of being acted on by light, if for any cause the illumination of the surrounding surface is delayed or if the most sensitive part of the whole period corresponds to the ordinary darkness of night

6 *Experiments which show the Sensitive Condition during Stages* (ii) *and* (iii) —It was very important to obtain beyond any doubt the demonstration that the larvæ are sensitive during Stage (ii), and also to decide conclusively whether any susceptibility was continued into Stage (iii), and if so to compare the relative susceptibilities of the two stages Such experiments, if successful, would at once dispose of the older theory of pupal sensitiveness, and would be most important in making possible other methods of investigation which, applied to Stage (iii) alone, might successfully terminate the long and difficult search for the larval sensory surface which is affected by surrounding colours A great many experiments were conducted with this object The larvæ were made to pass Stages (i) and (ii) exposed to the influence of a powerfully acting colour, and then were transferred for Stage (iii) to the colour which tended most strongly in the opposite direction The largest, most carefully conducted, and most successful experiment of the kind gave the following results, all the larvæ belonging to the same company —

Degrees of colour	(1)	(2)	Dark (3)	(3)	Light (3)	(4)	(5)	
In black surroundings for the whole period		1	.	5	..	1		= 7
Transferred from black into gold for Stage (iii)				1	5	3	.	= 9
Transferred from gold into black for Stage (iii)					6	9		=15
In gold surroundings for the whole period			..		5	7	8	=20
								51

The analysis speaks for itself Stages (ii) and (iii) are both sensitive, but Stage (iii) is much less sensitive than the other Thus, when the earlier part of the period was passed in gilt surroundings, the resemblance between the results and those produced by gilt surroundings acting during the whole period was much stronger than the resemblance between the latter and the results produced when the gilt acted during Stage (iii) only It is observable that the larvæ as a whole evidently tended towards the lighter forms, so that the black did not produce nearly such strong effects as the gilt surroundings It is almost unnecessary to point how completely the old theory of pupal sensitiveness is broken down by the analysis The experiment shows that the more elaborate methods alluded to above could be applied to Stage (iii) with at any rate a fair prospect of success

7 *The Search for the Sensitive Larval Surface* (*a*.) *The Ocelli* — The most obvious suggestion pointed towards the larval ocelli (six in number on each side of the head) as the possible sense organs which were acted on by surrounding colours, and formed the beginning of the physiological chain of which the end is seen in the colours of the pupa In many different experiments the larvæ were divided into two sets, with precisely similar conditions of surrounding colours and illumination, the one set of larvæ being normal while the ocelli of the other larvæ were carefully covered with an opaque black varnish, which was renewed more than once if necessary (the larvæ being very much irritated by the process and flinging their heads about so as to remove some of the varnish) The material made use of was a quickly-drying photographic varnish rendered opaque by the addition of lamp black Experiments of this kind were conducted with green, white and gilt surroundings but the pupæ which were formed from the blinded larvæ could never be distinguished as a whole from the others having been equally acted upon by surrounding colours Even supposing the conditions of experiment had not been quite perfect, so that the ocelli were not wholly eliminated, we should expect some differences between the resulting pupæ if these organs represent the efficient sensory surface After repeated experiments with negative results I subjected two sets of larvæ to the influence of black surroundings in darkness thinking it possible (but highly improbable) that the process of blinding, or the varnish itself might act as a stimulus to the ocelli, and so produce the light-coloured pupæ Again it was possible that the blinding might assist the influence of black surroundings, although it could not prevent the action of bright colours Of the resulting pupæ, the set produced from the blinded larvæ were rather lighter than the others, but there was little difference, and hence both suggestions were negatived, for the process obviously did not assist the influence of the surroundings, and the difference between the two sets was so slight as to offer no explanation of the brilliant pupæ produced from blinded larvæ by gilt or white surroundings, on the hypothesis that the process of blinding itself supplies a stimulus

(*β*.) *The Complex Branching Spines* —It seemed possible that these spines, of which there are seven on most of the segments, might contain some terminal organ which receives impression from coloured surfaces When the spines are snipped off the bases bleed a little, so it is clear that a subcuticular core is contained within them The bristles were shorn from several mature larvæ, and they were placed under exactly the same conditions of light and surroundings (white or gilt being used in three different experiments) as about an equal number of normal larvæ, but the pupæ of the two sets were almost exactly similar.

(γ.) *The whole Skin Surface as Tested by Conflicting Colour Experiments.*—It has been shown in paragraph 6 that the larvæ are to some extent sensitive during Stage (iii), and I had long thought that this stage in which the larvæ are suspended motionless, and cannot be greatly affected by disturbance, might be investigated by the application of strongly conflicting colours to different parts of the larval surface. Black and gilt were obviously shown to be the best colours to select for the purpose, and the experiments were conducted in two ways. In the first the larvæ were induced to suspend themselves from sheets of clear glass, by placing them in wide shallow glass boxes, so that the ascent to the glass roof was easily accomplished. As soon as suspension had taken place each of the larvæ was covered with a compartmented cardboard tube, of which the septum was perforated by a hole just large enough to admit the larval body. The tube was fixed to the glass sheet with glue, and the upper chamber and upper surface of the septum were lined with one colour, *e.g.*, gold, while the lower chamber and lower surface of the septum were lined with the opposite colour, *e.g.*, black, which also covered the outside of the cylinder, in case the larva should stretch beyond its lower edge. The septum was placed at such a height in the tube that the larval head and rather less than half of the total skin surface (anterior) were contained in the lower chamber, while rather more than half of the skin surface (posterior) was contained in the upper chamber. It was thought that the upper chamber would be illuminated too strongly as compared with the lower, because its opening was directed upwards towards the light descending from the window, and therefore compensation was provided by fixing another perforated septum on the upper end of the cylinder, so that its opening was reduced to the same diameter as the perforation in the septum between the two chambers. The results show that I overcompensated for the difference in illumination, for I did not take into account the fact that the larva spins its boss on a comparatively wide layer of silk which it has previously spun over the glass, and which greatly diminishes the transparency of the latter over an area at least equal to the diameter of the tube, which is comparatively thick, and includes the boss itself over the smaller area, corresponding to the perforation in the disk. Hence the resulting pupæ were rather lighter when the gilt chamber was below, although the difference was not great. At the close of the experiment I altered the conditions of illumination by removing the upper septum, and then the single pupa produced in a tube with the gilt chamber above proved to be the only (5) obtained in the whole of this set of experiments, in which 83 pupæ had been compared. Such results show that the sensitive surface is not represented by a sense-organ in the head, or with an anterior portion only, but that the whole skin area possesses susceptibility.

The second method of conducting conflicting colour experiments was superior in the more equal illumination of the gilt surface when above or below. Flat wooden trays were covered in each case with black and gilt paper in alternating areas, the two colours meeting along lines which ran across each tray, and along which shelves were fixed covered with gilt paper towards the gilt surface, and black paper towards the black surface. The shelves were perforated close to the tray bottom with holes separated by equal distances, and of such a size as to easily admit the body of a larva with its spines, while the latter as in the compartmented tubes tend to obscure the interval between the larval body and the edge of the aperture. The trays were placed vertically in a strong east light, so that the shelves projected horizontally, the black surface being uppermost in some cases, the gilt surface in others. Suspended larvæ were pinned (by the boss of silk) on to the uppermost colour in such a position over the holes that the head and first five segments of each larva passed through a hole into the colour beneath, which tended to produce opposite results. The curvature of the larval body brought the head close up to the underside of the shelf, and thus there was no chance of its being influenced by the colour above the shelf. Other larvæ were similarly fixed between the shelves upon one colour only, so as to afford a comparison with the results of the conflicting colours. The pupæ obtained were on the whole rather lighter when the gilt surface was above, and hence the gilt surroundings influenced the rather larger posterior part of the skin to a greater extent than in the converse arrangement, when the effective colour was below. Hence on the whole the influence of conflicting colours has ended in as complete a confirmation of the numerous blinding experiments as the necessarily limited conditions of experiment could be expected to produce.

8. *The Nature of the Effects Produced.*—The gilded appearance is one of the most metal-like appearances in any non-metallic substance. The optical explanation has never been understood. It has, however, been long known that it depends upon the cuticle, and needs the presence of moisture, and that it can be renewed, when the dry cuticle is moistened. Hence it can be preserved for any time in spirit. If a piece of dry cuticle be moistened on its upper surface the colour is not renewed, but almost instantly follows the application of spirit to the lower surface. Sections of the cuticle resemble those of *Papilio machaon* described in a previous paper ('Roy. Soc. Proc.,'

vol 38, 1885, p 279), and show an upper thin layer, and a lower much thicker, finely laminated layer, which is also striated vertically to the surface With Professor Clifton's kind assistance I have been able to show that the appearances follow from interference of light, due to the presence of films of liquid between the lamellæ of the lower layer The microscope shows brilliant red and green tints by reflected light, while in transmitted light the complementary colours are distinct, but without brilliancy The latter colours are seen to change when pressure is applied to the surface of the cuticle, and when the process of drying is watched under the microscope, owing in both cases to the liquid films becoming thinner In the dry cuticle the solid lamellæ probably come into contact, and prevent the admission of air, which, if present, would cause even greater brilliancy than liquid The spectroscope shows broad interference bands in the transmitted light, which change their position on altering the angle of incidence of the light which passes through the cuticle Precisely similar colours, metallic on reflection, non-metallic and with the complementary tints on transmission, with the same spectroscopic appearances and changes induced by the same means, are seen in the surface films which are formed on bottle glass after prolonged exposure to earth and moisture In the alternating layers of the pupa the chitinous lamellæ are of higher, the liquid films of lower refractive index, hence water or alcohol produce brilliant appearances, while liquids of higher refractive indices produce less effect.

It is very interesting to note that this most specialised means of producing colour is probably derived in the most simple manner from the ordinary lamellated layer of other non-metallic pupæ (*e g*, *P machaon*) in which the lamellæ merely act as reflectors so that the pupa is brightly coloured by absorption due to pigment contained in the outer lamellæ only, and hence traversed twice by a large part of the incident light

The dark pupæ of *V urticæ*, and the dark parts of the brilliant pupæ, contain abundant pigment in the upper thinner layer only, which therefore acts as a screen, and shuts off light from the lamellated layer below, thus preventing the metallic appearance In the brilliant pupæ this layer is transparent, and of a bright yellow colour, and doubtless assists in producing the yellowness of the golden appearance by absorption of light The two layers are of different chemical constitution, for the upper will not stain in logwood, while the lower does so without difficulty

9. *The Biological Value of the Gilded Appearance* —Mr T W Wood suggested that the appearance was so essentially unlike anything usually found in the organic kingdoms as to protect the organisms possessing it Others have thought that it has the value of a warning colour, indicating an unpleasant taste It is probable that it is now

used for this purpose, but it is improbable that such was its original meaning, for the fact that the appearance can be called up by the appropriate surroundings shows that it belongs to the highest class of protective colours, as far removed as possible from conspicuous warning colours, the object of which is to become as *unlike* their surroundings as possible. The former suggestion no doubt contains the true origin of the character, if we add to it the statement that the appearance is not only unlike anything organic, but strongly resembles many common mineral substances, especially the widespread mineral mica. The darker pupæ, on the other hand, resemble grey and weathered rock surfaces, just as the brilliant varieties resemble many exposed and recently fractured rocks. The shape of the Vanessa pupa is eminently angular and mineral looking. It is probable that the glittering form arose in a hot dry country, where exposed rocks would not weather for a long period of time. Gilded pupæ of Vanessa are formed from larvæ which contain parasitic larvæ of ichneumon flies, probably on account of the absence of pigment in such diseased individuals, and such absence being correlated with the gilded appearance, the latter is therefore formed. *Vanessa Io* has a green variety of pupa which appears when the insect is attached to its food-plant; *V. atalanta* has not such a form, and spins a tent of leaves when it pupates on the plant, while *V. urticæ* has neither the green variety nor the latter habit, and exhibits a strong disinclination to pupate among vegetal surroundings. During the past summer I only found three pupæ of the species on the food-plant in the field, and *all were* "*ichneumoned*" and were abnormally gilded.

III. *Experiments upon Vanessa atalanta.*—A few larvæ of this species, kindly sent me by Mr. J. L. Surrage, were subjected to gilt and to black surroundings, while a few others were left in bright light among the leaves of the food-plant. The results harmonised very completely with those obtained from *V. urticæ*, the first set of pupæ being uniformly golden, the second very dark and with hardly any or none of the gilded appearance, while the third were intermediate but nearer to the former. The length of Stage (iii) appeared to be about the same as in *V. urticæ*, as far as this could be ascertained from the limited data.

IV. *Experiments upon Papilio machaon.*—Mr. W. H. Harwood supplied me with larvæ of this species. The eight largest were selected and placed in brown surroundings (twigs, &c.), four of them being blinded. The larvæ were very quiet and did not appear to be irritated by the process, which was repeated three times. The position of the ocelli on a distinct black area rendered it easy to ensure that they are all covered with varnish. Eight bright green pupæ were obtained, fixed to the brown stems or roof or lying free on the brown floor. This result surprised me very much, for I knew that there was a brown variety of the pupa not uncommon in this species The remaining three larvæ were placed in green surroundings, one of them being blinded as above, but only one of the normal larvæ pupated, fixed to the green food plant, and produced a *distinct brown variety* These startling results show that there can be no susceptibility in this species, and this is all the more remarkable because the two varieties are so well marked, and because of the striking results obtained by Mrs Barber and other observers on two species of South African Papilios Fritz Muller, however, shows that another species of this genus resembles *P machaon* in being dimorphic and yet not susceptible The contradictory results obtained in my experiments were either due to the secondary association of one variety of a dimorphic species with an unhealthy condition or even a stunted size, as the gilded Vanessa pupæ result from 'ichneumoned" larvæ, or to the shade caused by the green tissue paper The eight largest and healthiest larvæ produced the green pupæ, while of the three smaller larvæ only one pupated and formed a brown pupæ Mr Harwood informs me that he has always looked with suspicion on the brown pupæ, believing that they have been bred from larvæ which were captured when small, and which are reared in close fitting tin boxes and he believes that the wild pupæ, and those obtained from larvæ which were found when almost mature, are green On the whole I think it is probable that the pupal dimorphism in this species is the remnant of a former susceptibility to coloured surroundings

V *Experiments upon Pieris brassicæ and P rapæ*—These two species are treated together because they were in nearly all cases kept under similar conditions and were often placed in the same cylinders The (nearly mature) larvæ were almost always obtained, and the experiments conducted, at Seaview (Isle of Wight)

1 *Standards of Pupal Colour*—Degrees of colour were constructed by the comparison of a large number of individuals in each species In these standard lists the pupæ were arranged in both species according to the relative predominance of black pigment, both as patches and minute dots, the latter tending to produce a grey appearance and obscuring the ground colour The lightest degrees were classified according to the tint of the ground colour which had become prominent in the comparative absence of the pigment

2 *Effects of various Colours acting during the Preparatory Period*

(*a*) *Black*—Interesting results following the use of this black ground under various conditions of illumination (*P rapæ* only), the effects being stronger in the direction of pigment formation when the amount of light was increased (the opposite effect having been witnessed in *V urticæ*) The pupæ of both species were dark in the great majority of instances after exposure to black surroundings in the larval state during the preparatory period (β.) *White.*—In this case also the effects were stronger (as shown in the prevention of pigment formation) as the surface was more highly illuminated (*P. rapæ* only).

(γ.) *Colours of the Spectrum.*—All the colours were used except violet, and the effects upon pigment formation in the two species were so graduated in the successive colours that it was possible to approximately represent the results by a graphic method, making the abscissæ of the scale of wave-lengths of the visible spectrum, and each ordinate of a length which corresponded to the average amounts of pigment obtained from all the pupæ subjected to any one colour, each ordinate being made to diverge at its base, and to include the degrees on the scale of wave-lengths which were shown by the spectroscope to correspond to the rays reflected (or transmitted) by the colour in question. Joining the summits of all the ordinates, the lines obtained were strikingly similar in the two species.

The effects may be summarised as follows:—

Colours.	*P. brassicæ.*		*P. rapæ.*	
Black (*for comparison*).	Largest amount of pigment.		Largest amount of pigment.	
Dark red.	Almost the same.		..	
Deep orange.	Smallest amount of pigment.	Marked effects on tint of ground colour, especially in the case of *orange*	Smallest amount of pigment.	Marked effects on tint of ground colour.
Pale yellow.	Rather more.		Rather more.	
Green.	Rather more.		Intermediate between the two last	
Pale bluish-green	Much more, almost equal to *black* and *red*.		More than in *yellow*.	
Dark blue.	.. ..		Still greater amount, but not nearly equal to *black*.	

The colours which most retard the formation of pigment were shown by the spectroscope to contain certain rays in common, *i.e.*, those from W.L. 0·00057—W.L. 0·00059, or 0·00060. The whole of the experiments on these species seemed to show that, of the light incident on the larval surface. the direct white light produces no effect at all (until after it has been reflected). Further experiments must decide whether direct light can be equally efficient with reflected light, when it contains the same spectroscopic components. The green tissue-paper was quite insufficient to prove this, for it must have been largely coloured by absorption from reflected as well as transmitted light.

3 *The Length of the Preparatory Period*—The observations were not sufficient to determine the duration of the periods and of its stages with any great accuracy, but all the experiments render it certain that the length is much greater in both species than in *V urticæ* There were also some indications as in *V urticæ*, that darkness may cause the prolongation of the period

4 *Blinding Experiments*—The larvæ of *P rapæ* were alone made use of, and they are as well suited to this method of investigation as *P machaon* The sets of pupæ produced from normal and blinded larvæ were very similar and thus the results harmonise with those of all the blinding experiments in other species of larvæ

5 *Transference Experiments*—A considerable number of the larvæ of *P rapæ* were transferred for the whole or part of Stage (iii) to a surface of a colour different from and generally opposite to that which had previously influenced them, and the results entirely harmonised with those previously described in other species, showing that the larva is sensitive and not the pupa, and that the time of greatest susceptibility is before Stage (iii), or only including the first part of it, but also rendering it probable that the larvæ can be influenced to a small extent during this stage

6 *The Nature of the Effects wrought upon the Pupæ*—The varied pigment effects which follow the influence of different surrounding colours are attended by other more deeply seated changes of even greater physiological interest and importance The black pigment patches and minute black dots are cuticular and superficial, while the green, pink, or other ground colours are subcuticular and deep seated, and in the most brightly coloured pupæ they are mixed colours due to the existence of different pigmentary (and probably chlorophylloid) bodies present in different elements and at different depths in the subcuticular tissues of the same pupa In other pupæ no trace of such colours can be seen Hence we see in these most complex and varied effects of the stimulus provided by the reflected light, which deepen into their permanent pupal condition very many hours after the stimulus has ceased to act, the strongest evidence for the existence of a chain of physiological processes almost unparalleled in intricacy and difficulty, while a theory of comparatively simple and direct photochemical changes induced by the stimulus itself, without such a physiological circle, seems entirely inadequate as an explanation of the facts, a conclusion which is borne out by a comparison with the experiments upon other species described in this paper

VI *Experiments upon Ephyra pendularia*—After the consideration of the many species of variable pupæ of the Rhopalocera, it is of interest to compare the results of the investigation of the equally exposed and variable pupæ of certain species of a single genus of Heterocera, the genus Ephyra. I observed this genus in 1883 (*i.e., E. pendularia, E. omicronaria*, and *E. orbicularia*), and the results are published in 'Entom. Soc. Trans.,' 1884, pp. 50—56. The most curious result of the observation was the establishment of the fact that the green and brown larvæ always produce pupæ of the same colour. I think it is very probable (from the consideration of other partially published observations), although entirely untested in this genus, that the colours of the larvæ, and through them of the pupæ, could be controlled by the selection of appropriate surroundings during the whole or a large part of the larval stage. Concerning the different species made use of, *E. orbicularia* is variable, *E. pendularia* regularly dimorphic green and brown, and *E. omicronaria* dimorphic, with the brown forms very rare. The relative numbers of the green and brown larvæ and pupæ of *E. pendularia* vary at different times of the year, the green forms greatly predominating in the summer brood, while they are not so abundant in the winter brood. When the parents of any set of larvæ were both of the same colour in the larval stage there was a much larger proportion of that same colour in the resulting offspring. I made some observations upon the situations selected for pupation, thinking that those might show some relation to the pupal colours, but the results were not convincing, and were certainly highly irregular, but the experiment was not carried out in the best way, for there was not a sufficient quantity of *both* colours in the surroundings. Dr Wilhelm Müller, of Greifswald (Spengel, 'Zool. Jahrb.,' vol 1, 1886, p. 234), calls attention to this remarkable and constant relation of larval to pupal colours, and expresses the belief that it is entirely exceptional, a statement which is of importance, when it is remembered that Dr. Müller has worked carefully for many years on the South American larvæ. Hence certain species of Ephyridæ afford an interesting contrast with all the other species of exposed pupæ which have been hitherto observed.

VII *Experiments upon the Colours of the Cocoon in Saturnia carpini.*—At the suggestion of Mr. W. H. Harwood I made some experiments upon this species, and found that four cocoons which were spun in the corners of black calico bags were very dark brown in colour, while those of other larvæ which had been freely exposed to light until after they had begun to spin, and which were not surrounded by dark surfaces, were nearly all perfectly white, and when darker of a much paler tint, and very different from the four mentioned above. Thus Mr. Harwood's suggestion seems to be entirely confirmed, and another instance of the influence of surroundings is added, and one which it appears cannot be explained in any way except by the supposition of the existence of a complicated physiological, and apparently a nervous circuit.

Presents, February 10, 1887.

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February 17, 1887

Professor STOKES D C L, President in the Chair

The Presents received were laid on the table, and thanks ordered for them

The following Papers were read —

I "A Record of Experiments upon the Functions of the Cerebral Cortex' By VICTOR HORSLEY, M B, F R.C S, F R S, Professor Superintendent of the Brown Institution, and EDWARD ALBERT SCHAFER F R S Jodrell Professor of Physiology in University College, London (From the Physiological Laboratory of University College) Received February 5 1887

(Abstract)

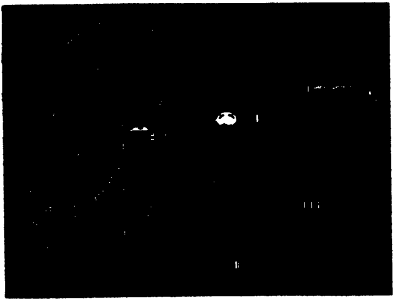
The paper consists as its title implies, of a record of experiments relating to the functions of the cerebral cortex, a subject upon which the authors have been engaged during three years The experiments have been entirely made upon monkeys After describing the methods employed, the general results of excitation and of extirpation of various parts of the cerebral hemispheres on one or both sides are given, and the cases in which the method of ablation has been employed are then recorded in detail, the symptoms observed during life and the condition of the brain after death being systematically noted Each case is illustrated by one or more drawings, showing the exact condition of the brain *post mortem* In some instances sections of the brain are also represented The paper includes also a topographical plan of the excitable or motor region of the *cortex cerebri*

II. "On Radiant Matter Spectroscopy —Examination of the Residual Glow" By WILLIAM CROOKES, F R S, V P C S Received February 10, 1887

The duration of phosphorescence after cessation of the exciting cause is known to vary within wide limits of time, from several hours in the case of the phosphorescent sulphides to a minute fraction of a second with uranium glass and sulphate of quinine In my examinations of the phosphorescent earths glowing under the excitement of the induction discharge in vacuo, I have found very great differences in the duration of the residual glow Some earths continue to phosphoresce for an hour or more after the current is turned off, while others cease to give out the light the moment the current stops Having succeeded in splitting up yttria into several simpler forms of matter differing in basic power,[*] and always seeking for further evidence of the separate identity of these bodies, I noticed occasionally that the residual glow was of a somewhat different colour to that it exhibited while the current was passing, and also that the spectrum of this residual glow seemed to show, as far as the faint light enabled me to make out, that some of the lines were missing This pointed to another difference between the yttrium components and with a view to examine the question more closely I devised an instrument similar to Becquerel's phosphoroscope, but acting electrically instead of by means of direct light

The instrument, shown in fig 1, A and B, consists of an opaque disk, *a b c*, 20 inches in diameter, and pierced with twelve openings near the edge as shown By means of a multiplying wheel, *d*, and

Fig 1 A



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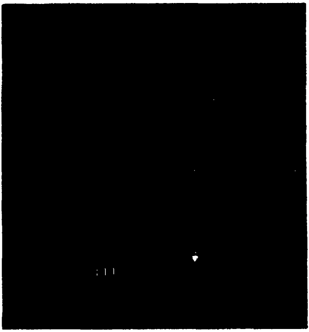
[*] Roy Soc Proc vol 40 pp 502 509 (June 10 1886)

Fig 1, B



band, *e f*, the disk can be set in rapid rotation At each revolution a stationary object behind one of the apertures is alternately exposed and hidden twelve times A commutator, *g* (shown enlarged at fig 1, B), forms part of the axis of the disk The commutator is formed of a hollow cylinder of brass round a solid wooden cylinder The brass is cut into two halves by a saw cut running diagonally to and fro round it, so as to form on each half of the cylinder twelve deeply cut teeth interlocking, and insulated from these on the opposing half cylinder by an air space about 2 mm across Only one half *h h h*, of the cylinder is used, the other, *i i i* being idle, it might have been cut away altogether were it not for some little use that it is in saving the rubbing spring, *j*, from too great friction when passing rapidly over the serrated edge To a block beneath the commutator are attached two springs, one, *k*, rubbing permanently against the continuous base of the serrated hemicylinder, *h h*, and the other, *j*, rubbing over the points of the teeth of *h h* By connecting these springs with the wires from a battery it will be seen that rotation of the commutator produces alternate makes and breaks in the current The spring, *j*, rubbing against the teeth is made with a little adjustment sideways, so that it can be said to touch the points of the teeth only, when the breaks will be much longer than the makes, or it can be set to rub near the base of the teeth, when the current will remain on for a much longer time and the intervals of no current will be very short. By means of a screw, *l l*, attached to the spring, any desired ratio between the makes and the breaks can be obtained. The intermittent primary current is then carried to an induction coil, *m*, the secondary current from which passes through the vacuum tube, *n*, containing the earth under examination. When the commutator, the coil-break, and the position of the vacuum tube are in proper adjustment, no light is seen when looked at from the front if the wheel is turned slowly (supposing a substance like yttria is being examined), as the current does not begin till the tube is obscured by an intercepting segment, and it ends before the earth comes into view. When, however, the wheel is turned more quickly, the residual phosphorescence lasts long enough to bridge over the brief interval of time elapsing between the cessation of the spark and the entry of the earth into the field of view, and the yttria is seen to glow with a faint light, which becomes brighter as the speed of the wheel increases.

To count the revolutions, a projecting stud, *o*, is fastened to the rotating axis, and a piece of quill, *p*, is attached to the fixed support, so that at every revolution a click is produced. With a chronograph watch it is easy in this way to tell the time, to the tenth of a second, occupied in ten revolutions of the wheel.

Under ordinary circumstances it is almost impossible to detect any phosphorescence in an earth until the vacuum is so high that the line spectrum of the residual gas begins to get faint; otherwise the feeble glow of the phosphorescence is drowned by the greater brightness of the glowing gas. In this phosphoroscope, however, the light of glowing gas does not last an appreciable time, whilst that from the phosphorescent earth endures long enough for it to be caught in the instrument. By this means, therefore, I have been able to see the phosphorescence of yttria, for example, when the barometer gauge was 5 or 6 mm. below the barometer.

When the earth under examination in the phosphoroscope is yttria free from samaria, and the residual emitted light is examined in the spectroscope, not all the bands appear at the same speed of rotation. At a slow speed the double greenish-blue band of Gβ (545) first comes into view, closely followed by the deep blue band of Gα (482). This is followed, on increasing the speed, by the bright citron band of Gδ (574), and at the highest speed the red band of Gζ (619) is with difficulty seen.

The following are measurements of the time of duration of the phosphorescences of the different constituents of yttrium. The wheel was first rotated slowly, until the first line visible in the spectroscope attached to the phosphoroscope appeared; the speed was counted, and it was then increased until the line next visible was seen. In this way the minimum speed of revolution necessary to bring each line into view was obtained, and from these data the duration of phosphorescence for each constituent of yttria was calculated. The time in the following table represents in decimals of a second the time elapsing between the cessation of the induction discharge and the visibility of the residual glow of the earth:—

At 0·0035 sec. interval the green and blue lines of Gβ and Gα begin to be visible.
At 0·0032 „ the citron line of Gδ begins to be visible.
At 0·00175 „ the deep red line of Gζ (647) is just visible.
At 0·00125 „ the line of Gδ is almost as bright as that of Gβ, and the red line of Gη is visible.
At 0·000875 „ the highest speed the instrument could be revolved with accuracy, the whole of the lines usually seen in the yttria spectrum could be seen of nearly their usual brightness.

I have already recorded* that phosphate of yttria, when phosphoresced in vacuo, gives the green lines very strongly whilst the citron band is hazy and faint. The same tube of yttric phosphate was now examined in the phosphoroscope. The green lines of Gβ soon showed themselves on setting the wheel into rapid rotation, but I was unable to detect the citron band of Gδ even at a very high speed.

The effect of calcium on the phosphorescence of yttria and samaria has been frequently referred to in my previous papers. It may save time if I summarise the results here. About 1 per cent. of lime added to a badly phosphorescing body containing yttrium or samarium always causes it to phosphoresce well. It diminishes the sharpness of the citron line of Gδ but increases in brightness. It also renders the deep blue line of Gα extremely bright. The green lines of Gβ are diminished in brightness. Lime also brings out the phosphorescence of samarium, although by itself, or in the presence of a small quantity of yttrium, samarium scarcely phosphoresces at all.

In the phosphoroscope the action of lime on yttrium is seen to entirely alter the order of visibility of the constituents of yttrium. In a mixture of equal parts yttrium and calcium, the citron Gδ line is the first to be seen, then comes the Gα blue line, then the Gβ green line, and finally the Gη red line. This may, I think, be explained somewhat as follows:—Calcium sulphate has a long residual phosphorescence, whilst yttrium sulphate has a comparatively short residual phosphorescence. Now with yttrium, although the green phosphorescence of Gβ lasts longest, it does not last nearly so long as that of calcium sulphate. The long residual vibrations of the calcium compound induce, in a mixture of calcium and yttrium, phosphorescence in those yttric molecules (Gδ) whose vibrations it can assist, in advance of those (Gβ) to which it is antagonistic; the line of Gδ therefore appears earlier in the phosphoroscope than that of Gβ, although were calcium not present the line of Gβ would appear first.

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* 'Phil. Trans.,' 1883, Part III (pp. 914—916).

Experiments were now tried with definite mixtures of yttria and lime as ignited sulphates, to see where the special influence of lime on Gδ ceased.

Yttrium.	Calcium.	
Per cent.	Per cent.	
97½	2½	Order of appearance in the phosphoroscope.—Gβ, Gα, Gδ, and Gη. The citron line of Gδ is only to be seen at a high speed, and is then very faint.
95	5	Order of appearance in the phosphoroscope.—Gα, Gβ, and Gδ (citron and blue) together, and lastly Gη (red). At a very high speed the green lines of Gβ become far more luminous than any other line.
90	10	Order of appearance.—Gδ and Gα together, then Gβ, and lastly Gη.
80	20	Order of appearance —Gδ and Gα simultaneously, then Gβ, and lastly Gη. The residual phosphorescence last for 30 seconds after the current stops. The light of this residual glow is entirely that of Gδ. The line of Gβ comes into view at an interval of 0·0045 second. At 0·00175 second the line of Gη is just visible.
60	40	Order of appearance.—Gδ and Gα together, then Gβ and Gη together
50	50	
40	60	
30	70	Order of appearance.—Gδ, Gα, Gβ.
10	90	
5	95	
1	99	Order of appearance.—Gδ, Gα. The green lines of Gβ could not be seen in the phosphoroscope; they would probably be obliterated by the stronger green of the continuous spectrum given by the calcium.

The action of barium on yttrium was now tried. The following mixtures (as ignited sulphates) were made:—

Yttrium.	Barium.	
Per cent.	Per cent.	
95	5	In the phosphoroscope the Gβ line appears earliest, but the blue Gα line is the next to be seen, whilst the red line of Gη is the latest in appearing. As the percentage of yttrium increases the blue line more and more overtakes the red and increases in brightness.
90	10	
80	20	
70	30	Spectrum similar to the above. As the percentage of yttrium increases the spectrum grows brighter. In the phosphoroscope the earliest line to appear is the Gβ green, then the Gη red, and next closely following it the Gα blue
60	40	
50	50	
40	60	
30	70	
25	75	

Yttrium.	**Barium.**	
Per cent.	*Per cent.*	
20	80	In the radiant matter tube all these mixtures give similar spectra. The Gβ green is a little brighter and the Gδ citron is a little fainter than in the corresponding mixtures of yttrium and calcium, but the whole of the yttrium lines are seen. In the phosphoroscope the Gβ green is the first to appear, then the Gη red. The Gδ citron is not visible at any speed
15	85	
10	90	
5	95	
1	99	Red line of Gη is much brighter; Gδ is very faint, and the green of Gβ is stronger In the phosphoroscope the order of appearance is,—first the line of Gβ, then the red line of Gη.
0·5	99·5	Phosphoresces with difficulty, of a light blue colour, but turns brick-red in the focus of the pole. Spectrum very faint. Order of appearance to phosphoroscope:—Gβ first, the others too faint to be seen.

The next experiments were tried with strontium, to see what modification the addition of this body to yttrium would produce. The following mixtures of ignited sulphates were experimented with:—

Yttrium.	**Strontium.**	
Per cent.	Per cent.	
95	5	A very good *yttrium spectrum*. In the phosphoroscope the order of appearance is—First the green of Gβ, then the Gα blue, lastly the Gη red. No Gδ citron line could be seen.
80	20	In the phosphoroscope the green of Gβ is very prominent at a low speed, standing out sharply against a black background. With a higher velocity the Gα and Gη lines come into view
60	40	The ordinary spectrum of this and the neighbouring mixtures is very rich in the citron line of Gδ, but I entirely fail to see a trace of this line in the phosphoroscope at any speed. The line of Gβ is the first to come, then the blue line of Gα.
40	60	
35	65	At about this point a change comes over the appearance in the phosphoroscope. The blue line of Gα is now the earliest to appear, and it is followed by the Gη red and Gβ green. No Gδ line is seen.
25	75	These mixtures are very similar to each other in the phosphoroscope. The line of Gα comes first, next the Gη line, then Gβ line. No Gδ citron line has been seen in any of these mixtures.
15	85	
5	95	
0·5	99·5	

In a paper read before the Royal Society, June 18th, 1885[*], I described the phosphorescence spectrum given by a mixture of

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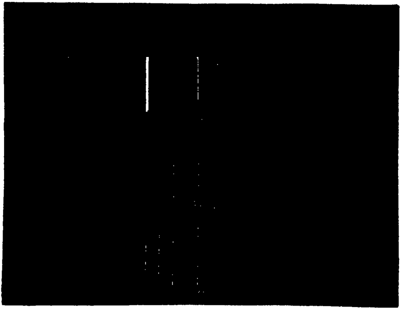
[*] 'Phil. Trans,' 1885, Part II (p. 716).

61 parts of yttrium and 39 parts of samarium, and illustrated it by a coloured lithograph Also in a paper read before the Royal Society, February 25th, 1886,* I described and figured the phosphorescent spectrum of an earth obtained in the fractionation of yttria which was identical, chemically and spectroscopically, with an earth discovered by M de Marignac, and provisionally called by him Yα I repeat here these spectra, with the spectrum of yttrium added for comparison Omitting minor details, it is seen that the Yα spectrum



is identical with that of the mixture yttrium 61, samarium 39, with one important exception—the citron line of Gδ in the former spectrum is absent in the latter Could I by any means remove Gδ from the mixture of yttrium and samarium the residue would be Yα I have little doubt that this will soon be accomplished, but in the meantime the phosphoroscope enables us to remove the line of Gδ from the mixture It is only necessary to add strontium to a suitable mixture of yttrium and samarium and view the phosphorescing mixture in the instrument when the wheel is rotating rapidly, to obtain a spectrum which is indistinguishable from that of Yα

In the search for bodies giving discontinuous phosphorescent spectra I have submitted a great number of earths and combinations to the electric discharge in vacuo, and have noted the results As the superficial phosphorescence apart from the composition of the emitted light has formed the subject of several recent papers by my friend M Lecoq de Boisbaudran, before the Académie des Sciences, it may be useful if I place on record some of the more striking facts which have thus come under my notice The bodies are arranged alphabetically, and, unless otherwise explained, were tested in the radiant matter tube in the form of ignited sulphates

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* 'Roy Soc Proc,' vol 40 p 236.

Alumina, in any of the forms which give the crimson line (λ6942—6937) has a very persistent residual glow In the phosphoroscope rubies shine with great brilliancy This phosphorescence of alumina has recently been the subject of a paper read before the Royal Society *

Antimony oxide with 95 per cent of lime (in the form of ignited sulphate) White phosphorescence, the spectrum showing a broad space in the yellow, cutting the red and orange off In the phosphoroscopes the residual glow is very strong, and of a greenish colour The spectrum of the residual light shows that the red and orange are entirely obliterated, leaving the green and blue very luminous Antimony oxide with 99 per cent of lime gives a pale yellowish phosphorescence, which on heating turns red In other respects it is like the 5 per cent mixture

Arsenious acid with 99 per cent of lime gives a greenish-white phosphorescence like pure calcium sulphate

Barium 5 per cent, calcium 95 per cent —The sulphates phosphoresce green, with specks of yellow and violet The spectrum is continuous, with slight concentration in the red, great concentration in the green, and in the orange a broad black band hazy at the edges

Bismuth 15 per cent, calcium 85 per cent phosphoresces of a bright reddish-orange The spectrum shows a tolerably sharp and broad dark band in the red and orange, and a strong concentration of light in the green and blue, the spectrum being continuous and divided into two parts by a black band in the yellow, as in the case of the antimony calcium spectrum In the phosphoroscope the red and orange disappear and the green and blue remain Bismuth 7 per cent, calcium 93 per cent —The action is similar to the 15 per cent mixture, except the colour of the phosphorescence, which is whiter In the phosphoroscope the red and orange below the dark band is cut off With 2 per cent of bismuth the same phenomena occur With 0 5 bismuth the phosphorescence is greenish blue and the spectrum is continuous, with strong concentrations in the orange and green The phosphoroscope cuts off the red and orange

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* Roy Soc Proc ' vol 42 1887 p 25

Cadmium 1 per cent, calcium 99 per cent—Similar to calcium sulphate, *q v*

Calcium sulphate was prepared from a colourless and transparent rhomb of Iceland spar which had been used for optical purposes It was dissolved in nitric acid, the nitrate was decomposed with distilled sulphuric acid, and the ignited sulphate tested in the tube The phosphorescence is bright greenish blue without bands or lines In the phosphoroscope the colour is a rich green the spectrum shows the red and orange entirely cut off, leaving the green and blue, the blue is especially strong

Calcium sulphates prepared from Professor Breithaupt's calcites* were re examined All phosphoresce with the normal greenish blue glow of calcium, except No 11, which gives a reddish glow A minute trace of samarium was found in this calcite, but not enough to affect the colour of the glow In the phosphoroscope all the specimens give a continuous spectrum beyond the yellow the red and orange being cut off as usual

Chromium 5 per cent, calcium 95 per cent, as sulphates, gives a pale reddish phosphorescence In the phosphoroscope the colour is green, and the red and orange are cut off 1 per cent of chromium with calcium phosphoresces green in the cold, and becomes a red when slightly heated The behaviour of chromium with aluminium has already been described †

Copper sulphate with 95 per cent calcium sulphate behaves like calcium sulphate

Diamonds phosphoresce of various colours Those glowing pale blue have the longest residual glow, next come those phosphorescing yellow I am unable to detect any residual glow in diamonds phosphorescing of a reddish colour A large diamond of a greenish hue, very phosphorescent shines almost as brightly in the phosphoroscope as out of it

Glucina phosphoresces of a rich blue colour There appears to be no residual glow with this earth in the phosphoroscope

Lanthanum—All the specimens of lanthanum sulphate I have examined in the radiant matter tube phosphoresce of a reddish colour and give a broad hazy band in the orange, with a sharp line —$1/\lambda^2$280—superposed on it This is identical with the line of Gα, one of the constituents of the samarium phosphorescent spectrum Calcium added to lanthanum changes the colour of the phosphorescence from red to yellowish, and brings out yttrium and samarium lines, these metals being present as impurities, the Gδ and Gα lines are also seen, but the space which should be occupied by the Gβ green is now a dark space I have shown that when Gδ, Gα, and Gβ are present in very small quantities with lime, the lines of $G\delta$ and $G\alpha$ are intensified, while that of $G\beta$ is weakened. This new result seems to show that if only a small trace of $G\beta$ is present with lime and lanthanum, the green line is not only suppressed, but the quenching action has actually extended so far as to neutralise that part of the continuous lime spectrum having the same refrangibility as the $G\beta$ line, the result being a black space in the spectrum. In the phosphoroscope the line of $G\epsilon$ is visible at the slowest speed, $G\delta$ comes in at an interval of 0·0035 second, and the $G\alpha$ line immediately afterwards.

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* Phil Trans 1885 Part II (p 697)

† Roy Soc Proc vol 42 p 28 *et seq*

Lead sulphate, by itself, in the radiant-matter tube glows with a nearly white colour, giving a continuous spectrum. In the phosphoroscope the red and orange are cut off, leaving a strong concentration of light in the green and blue. 5 per cent of lead added to calcium sulphate phosphoresces like lime.

Magnesia phosphoresces pink. 5 per cent with lime, as sulphates, give a greenish phosphorescence, with a tendency to turn red as the powder heats. As the Oriental ruby contains between 1 and 2 per cent of magnesia, a mixture was prepared of acetate of alumina with 2 per cent of magnesia, and tested after ignition. It gave no spectrum or lines. This was done to see if the crimson line of aluminium might be due to the presence of magnesia.

Nickel added to calcium sulphate in the proportion of 5 per cent. makes no alteration in the usual phosphorescent phenomena of calcium.

Potassium, 5 per cent., added to calcium sulphate gives a bright phosphorescence, and made the residual glow very persistent.

Samarium.—The phosphorescent behaviour of this body, alone and mixed with other substances, has been fully described in my paper on samarium.*

Scandium, either in the form of earth or sulphate, phosphoresces of a very faint blue colour, but the light is too feeble to enable a spectrum to be seen. Addition of lime does not bring out any lines.

Sodium sulphate mixed with an excess of calcium sulphate gives a greenish tinge to the usual colour of the phosphorescence. The sodium line is visible in the spectrum.

Strontia in the radiant-matter tube glows with a rich blue colour, showing in the spectroscope a continuous spectrum with a great concentration of light in the blue and violet. In the phosphoroscope the colour of the glow is bright green, showing in the spectroscope a continuous spectrum, with the red and blue ends cut off. A mixture of calcium sulphate with 5 per cent of strontium sulphate behaves like calcium sulphate alone.

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* Phil. Trans. 1885, Part II (pp. 709—721).

Thorium, as oxide or sulphate, refuses to phosphoresce, and the tube rapidly becomes non-conducting. A tube with thoria at one end and a phosphorescent earth such as lime or yttria at the other end, and furnished with a pair of poles near each end, at a particular exhaustion is non-conducting at the thoria end, while it conducts at the yttria end. If the wires of the induction coil are attached to the poles at the thoria end, no current will pass; rather than pass through the tube, the spark prefers to strike across the spark gauge—a striking distance of 37 mm.—showing an electromotive force of 34,040 volts. Without doing anything to affect the degree of exhaustion, on transferring the wires of the induction coil from the thoria to the yttria end, the spark passes at once. To balance the spark in air the wires of the gauge must be made to approach till they are only 7 mm. apart, equivalent to an electromotive force of 6440 volts; the fact of whether thoria or yttria is under the poles making a difference of 27,600 volts in the conductivity of the tube. The explanation of this action of thoria is not yet quite clear. From the great difference in the phosphorescence of the two earths, it is evident that the passage of the electricity through these tubes is not so much dependent on the degree of exhaustion as upon the phosphorogenic property of the body opposite the poles. This view is supported by the fact that the thoria may be replaced by a metal wire, when the same obstructive action will result.

Lime does not give phosphorescent properties to thoria, if this earth be pure, but it brings out the lines of yttrium and samarium which are almost always present in small quantities in thoria unless it has been specially purified.

Tin with 95 per cent. of lime gives the lime phosphorescence only.

Thulium and *erbium* together phosphoresce with a green light, giving the erbium spectrum already described before this Society.* There is, in addition, a faint blue line apparently double (see "Ytterbium"). The addition of lime causes the mixture to phosphoresce of a pale blue colour. The spectrum now shows a bright blue band, in the same position as the faint double blue band seen in the absence of lime. The blue line of Gα is also seen, and a faint line of Gδ. The deep red line of Gη, one of the constituents of the ordinary yttria spectrum, is prominent in this spectrum.

Tungsten and *uranium*, each mixed with 95 per cent. of lime, only give the lime spectrum.

Ytterbium.—I have not yet succeeded in preparing this body of trustworthy purity; but through the kindness of Professor Clève, M. de Marignac, and Professor Nilson I have been enabled to experiment with specimens of ytterbia prepared by these chemists.

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* 'Roy. Soc. Proc.,' vol. 40, p. 77, fig. 1 (January 7, 1886).

Professor Clève's ytterbia, in the form of sulphate, gives in the radiant-matter tube a blue phosphorescence, the spectrum of which shows a strong double blue band,[*] together with traces of the $G\delta$ and the erbia green lines. The addition of lime broadens the blue band and makes it single. Professor Clève writes that this ytterbia may contain some traces of thulia, perhaps also of erbia, but scarcely any other impurities. Measurements in the spectroscope give the following approximate results.

Scale of spectroscope	λ.	$\frac{1}{\lambda^2}$.	Remarks.
8·63	4626	4673	Commencement of first blue line. This edge is very hazy.
8·54	4574	4780	Centre of the first blue line.
8·45	4524	4885	End of first blue line.
8·44	4518	4898	Centre of dark interval between the two blue lines.
8·40	4475	4994	Centre of second blue line. This line is narrower than the first line.

The following are measurements taken with the mixture of this ytterbia and lime :—

Scale of spectroscope.	λ.	$\frac{1}{\lambda^2}$.	Remarks.
8·71	4674	4577	Up to this point there is the continuous spectrum of li-calcium. Here a black space commences.
8·515	4555	4819	Commencement of a hazy blue band.
8·475	4533	4865	End of hazy blue band. This band is of considerable brilliancy.

These blue bands are seen much fainter without lime, and are about as strong in the mixture of thulia and erbia with lime described above. I had ascribed them to ytterbia, when Professor Nilson kindly forwarded me a small specimen of ytterbia, considered by him perfectly pure, and used for his atomic weight determinations. This ytterbia gives absolutely no blue bands. The origin of these bands therefore remains uncertain.

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[*] This is the band spoken of in my Royal Society paper of 9th June last ('Roy. Soc. Proc.,' vol. 40, 1886, p. 507), provisionally called $S\gamma$, and ascribed to ytterbia. If it is not due to ytterbia it is evidence of a new body.

Ytterbia from *Professor* Nilson, in the *form* of sulphate, refuses to phosphoresce without the addition of lime. When lime is added it only brings out traces of the phosphorescent bands of $G\epsilon$, $G\beta$, and $G\alpha$. Evidently these are impurities.

Ytterbia from M. de Marignac is identical with that from M. Clève, as far as my examination can go. In sending me this ytterbia M. de Marignac warned me that he was very far from thinking it pure.

Yttrium.—During the fractionation of the higher fractions of yttria (+6, 118 and 119), a very sharp green line sometimes makes its appearance, situated between $G\beta$ and $G\gamma$ (approximate position on the $1/\lambda^2$ scale, 325). It is very faint, and is not connected with the orange line of $S\delta$, although it is as sharp. The yttria showing these lines phosphoresces of a transparent golden-yellow colour, the fractions at the other end phosphorescing yellowish green.

I have previously described the action of a large number of bodies on the phosphorescence of samarium.* The experiments resulting in the following observations were tried at about the same time. I will describe them in alphabetical order. Unless otherwise mentioned all the mixtures were in the form of anhydrous sulphates.

Yttrium 5 per cent., *aluminium* 95 per cent., gives a good yttria spectrum; the blue line of $G\alpha$ is very distinct, and the double green of $G\beta$ is well divided. In the phosphoroscope the $G\beta$ and $G\alpha$ lines first appear simultaneously, then the $G\delta$ line.

Yttrium 99·5 per cent., *bismuth* 0·5 per cent.—The spectrum is bright, and on close examination a trace of samarium green, $G\gamma$, is to be detected forming a wing to the $G\delta$ line. In the phosphoroscope the citron line of $G\delta$ entirely disappears and the samarium double green line, which out of the phosphoroscope is almost obscured by the great brightness of $G\delta$, now appears distinctly, together with the green $G\beta$ line. Yttrium 95 per cent., bismuth 5 per cent., gives the usual yttria spectrum. No $G\delta$ line appears in the phosphoroscope at any speed. At first only the $G\beta$ line is seen, and next the $G\alpha$ line appears, as in yttria. On gradually increasing the percentage of bismuth the spectrum of yttria grows fainter, until with 95 per cent. of bismuth the phosphorescence is bad and the spectrum faint.

Yttrium 5 per cent., *cadmium* 95 per cent., gives a brilliant phosphorescence, but the spectrum is almost continuous. In the phosphoroscope a faint concentration of light is seen in the green, which becomes sharper as the speed increases.

The action of calcium on the phosphorescence of yttrium has already been described.

Yttrium and *cerium*.—Cerium has the effect of deadening the brilliancy of the yttrium spectrum in proportion to the quantity added All the bands remain of their normal sharpness

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* "On Radiant Matter Spectroscopy. Part 2—Samarium." 'Phil. Trans.,' 1885, Part II (pp. 710—722).

Yttrium 5 per cent, *copper* 95 per cent, phosphoresces very feebly

Yttrium 90 per cent, *didymium* 10 per cent—This mixture gives a good yttria spectrum Yttrium 70 per cent, didymium 30 per cent, phosphoresces very fairly and gives all the usual lines

Yttrium 50 per cent, didymium 50 per cent, refuses to phosphoresce The tube is either too full of gas to allow the phosphorescence to be seen or it becomes non conducting When the mixture is illuminated by the glowing gas the absorption lines of didymium the green are seen With higher proportions of didymium the same results are produced On adding 25 per cent of lime to the mixture containing 50 per cent of didymium the yttria spectrum is brought out very well Lime added to a mixture of 10 per cent yttria and 90 per cent didymium brings out the yttrium spectrum fairly, but the tube soon becomes non conducting

Yttrium 5 per cent and *glucinum* 95 per cent gives a bright phosphorescence, but the definition of the spectrum lines of yttria is bad

Yttrium 5 per cent *thallium* 95 per cent—No spectrum is given by this mixture it turns black and refuses to phosphoresce

Yttrium 5 per cent, *tin* 95 per cent, phosphoresces faintly, the lines being very indistinct

Yttrium 5 per cent, *titanium* 95 per cent, acts like thoria, and the tube becomes non conducting

Yttrium 5 per cent *tungsten* 95 per cent—This phosphoresces of a bright yellow colour, the spectrum is brilliant but the lines are not sharply defined In the phosphoroscope the colour becomes greenish, and the spectrum shows only the green lines of Gβ

Yttrium 5 per cent, *zinc* 95 per cent—The phosphorescence is of a pale yellowish white, and the spectrum is very brilliant, being equal to that shown by 30 per cent of yttrium with barium calcium, magnesium, or strontium In the phosphoroscope the colour becomes reddish, and the Gβ green line is the first to come No citron line is seen If the yttrium contains a trace of samarium, the samarium spectrum, which is scarcely seen under ordinary circumstances, now comes out distinctly

Zinc sulphate mixed with 95 per cent of calcium sulphate phosphoresces a bright bluish green colour, the spectrum contains no bands or lines

Zinc sulphide (Sidot's hexagonal blende*)—This is the most brilliantly phosphorescent body I have yet met with In the vacuum tube it begins to phosphoresce at an exhaustion of several inches below a vacuum. At first only a green glow can be seen, as the exhaustion gets better a little blue phosphorescence comes round the edges At a high exhaustion, on passing the current the green and blue glows are about equal in brightness, but the blue glow vanishes immediately the current stops, while the green glow lasts for an hour or more In the phosphoroscope the blue glow is only seen at a very high speed, but the green glow is seen at the slowest speed, and the body is almost as bright in the instrument as out of it Some parts of a crystalline mass of blende which, under the action of radiant matter, leave a glow with a bright blue colour, leave a green residual light when the current ceases, other parts which glow blue become instantly dark on stopping the current

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* 'Comptes Rendus,' vol 62 1866, pp 999—1001; vol 63, 1866 pp 188—18

The different action of calcium, barium, and strontium on the constituents of yttrium is an additional proof, if confirmation be needed, that the bodies I have provisionally called Gα, Gβ, Gδ, &c.,* are separate entities It may be as well here to collect together the evidence on which I rely to support this view I will take the bodies *seriatim* —

Gα —An earth phosphorescing with a blue light, and showing in the spectroscope a deep blue line, of a mean wave-length 482 This earth occurs in different proportions in purified yttria from different minerals Samarskite, gadolinite, hielmite, monazite, xenotime, euxenite, and arrhenite contain most Gα, whilst fluocerite and cerite contained notably less of this constituent The addition of lime brings out the phosphorescence in Gα in advance of that of the other constituents The behaviour in the phosphoroscope of Gα when mixed with the alkaline earths also points to a difference between it and its associates With lime the blue phosphorescent band of Gα comes into view at a very low speed, the order of appearance with a small quantity of lime being Gβ, Gα, Gδ, and with a large quantity of lime, Gδ, Gα, Gβ Employing strontia instead of lime, the order of appearance in the phosphoroscope when the quantity of strontia is small is Gβ, Gα, Gη, and when the quantity of strontia is in excess, Gα, Gη, Gβ Baryta in small quantity brings out the lines in the phosphoroscope in the following order Gβ, Gα, Gη, but when the baryta is in excess the order is Gβ, Gη, Gα The chemical position taken up by Gα in the fractionation scheme precludes it from being due to the bodies I have called Gβ, Gγ, Gϵ, Gζ, Sγ, or Sδ It closely accompanies Gδ (the earth giving the citron line), concentrating at the least basic end, and I have not yet succeeded in effecting a separation of the two If therefore, Gα is not a separate entity, its blue line must be due to the citron-band-forming body called Gδ The difference between Gα and Gδ is brought out in a marked manner by the phosphoroscope when baryta or strontia is present, the citron line of Gδ being entirely suppressed while the blue line of Gα is brought out with enhanced brilliancy. For these reasons I am inclined to regard Gα as a separate body although the evidence in favour of this view is not so strong as in the case of some of its other associates.

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* 'Roy Soc Proc,' vol 40, 1886, p 502

Gβ.—An earth phosphorescing with green light, and showing in the spectroscope a close pair of greenish blue lines of a mean wavelength of 545. This earth can be separated by chemical fractionation from the other constituents of yttrium. It concentrates at the most basic end, and is present in the samarium which invariably makes its appearance at this end of the fractionation of yttrium. It is one of the prominent lines in Yα where also it accompanies some of the samarium lines. Gβ, however, is not a constituent of samarium, for it is easy to purify samarium by chemical means so that it does not show a trace of the Gβ green lines, although it is very difficult to get Gβ free from some of the samarium lines. The residual phosphorescence of Gβ is very considerable, and its green lines show first in the phosphoroscope when only yttrium is present. The addition of lime keeps back the glow of Gβ and brings forward that of Gε. Strontium and barium act on Gβ very differently to lime. A small quantity of strontium brings forward the residual glow of Gβ, whilst in large quantities strontium keeps the phosphorescence of Gβ back to the last.

Gγ.—An earth phosphorescing with a green colour, and showing in the spectroscope a green line having a wave length of 564. This is one of the least definite of all the supposed new bodies. It appears to be a constituent of samarium occurring in the fractionation of yttrium among the most basic constituents connecting yttrium and samarium. Its point of maximum intensity is chemically, very well marked, and is at a different part of the fractionation scheme to those of the other lines of samarium, especially Gε. On dilution with lime the phosphorescent line of Gγ vanishes before that of Gε.

Gδ.—An earth phosphorescing with a citron coloured light, and showing in the spectroscope a citron line having a wave length of 574. Gδ is one of the least basic of all the bodies associated in yttrium, occurring almost at one extremity of the fractionation. It is not very difficult to separate chemically Gδ from all the other accompanying bodies except the one which I have called Gα (giving the deep blue line). Not only can Gδ be obtained free from the other four constituents of yttrium, but the body called by M. de Marignac Yα is a proof that the other four components of yttrium can be obtained quite free from Gδ. Lime intensifies the phosphorescence of Gδ and deadens that of Gβ, while strontium has the opposite action. The behaviour of Gδ in the phosphoroscope, when mixed with lim strontia, or baryta, also affords a striking evidence of individuality, lime enhancing the residual glow, while strontia and baryta altogether suppress it.

Gε.—An earth phosphorescing with a yellow colour, and, in the spectroscope, showing a sharp yellow line having a wave-length of 597. It is seen in the samarium spectrum as a sharp yellow line superposed on a hazy double band. As I have already pointed out, Gε fractionates out high up among the most basic earths, and generally accompanies lanthanum. In the phosphorescent spectrum of lanthanum the line Gε is seen quite free from the lines of other bodies.

Gζ.—An earth phosphorescing with a red light, showing in the spectroscope a red line of wave-length 619 This body is always more plentiful in yttrium obtained from samarskite and cerite than from gadolinite, hielmite, and euxenite, and is almost absent in yttrium from xenotime. Gζ is of about intermediate basicity Working with samarskite yttria, Gζ becomes most brilliant after the line of Gη has completely disappeared Further fractionation causes the line of Gζ to fade out, and the citron and blue lines are then left.

The phosphorescence of Gζ is developed to a different extent according to the metal with which the yttria is mixed. The order (beginning with the substance having the greatest action) is zirconium, tin, aluminium, bismuth, glucinum.

Gη.—An earth phosphorescing with a deep red light, and showing in the spectroscope a red line having a wave-length of 647. Like its fellow red constituent, Gη occurs most plentifully in samarskite yttrium, and scarcely at all in yttrium from hielmite, euxenite, and cerite. It is the first of the strictly yttrium constituents to separate out, on fractionation, at the most basic extremity, leaving Gα, Gβ, Gδ, and Gζ. In almost all samples of yttria, except when very highly purified, Gη is seen very brilliantly, and by its side can be detected the faint red band of samarium In the phosphoroscope the line of Gη is the last to appear when yttria alone is being observed; strontia and baryta enhance the residual glow of Gη, strontia in moderate quantities bringing it out before that of Gβ, while baryta brings it out after Gβ.

Sδ.—An earth giving in the spectroscope when phosphorescing a very sharp orange line of wave-length 609. I have already* discussed the claims of this earth to be considered a separate entity. It is not present in the rare earths from gadolinite, xenotime, monazite, hielmite, euxenite, and arrhenite; it is present in small quantity in orite, and somewhat more plentifully in samarskite. In samarskite ttrium it concentrates at a definite part of the fractionation. Its sharp orange line is not strong enough to be seen in the phosphoroscope. A little calcium entirely suppresses the orange line, while samarium or yttrium seems to intensify it.

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* 'Roy. Soc. Proc.,' vol. 40, 1886, p. 540.

In addition to the above earths, it is not improbable that the sharp green line ($1/\lambda^2$ 325) mentioned under the heading "Yttrium" may be caused by still another earth.

The brilliant and characteristic spark spectra yielded when certain elements are volatilised and rendered incandescent by the spark from a powerful induction coil are relied on by chemists as an indisputable proof of the identity of such elements. Bearing this in mind I have endeavoured to ascertain how these yttrium constituents would behave in respect to the spark spectrum. Do the definite system of lines in the old yttrium spark spectrum belong to one constituent only, or are the yttrium lines broken up and distributed among the different bodies I have designated as Gα, Gβ, &c.? Also do the other constituents possess special spark spectra of their own? Very careful and long continued experiments have shown me that neither of these hypothetical cases occur.

The spark spectrum given by old yttrium is shown in the drawing (fig. 5). It is chiefly characterised by two very strong groups of lines in the red and orange. I now take the earth Gδ. This occurs near one end of the fractioning, and not only differs from the parent yttrium in its phosphorescent spectrum, but by virtue of the process adopted for its isolation it must likewise differ in its chemical properties. On examining its spark spectrum I see absolutely no difference between this spectrum and the one given by old yttrium.

FIG. 5.

I now pass to the other end of the fractionation of yttrium, where occurs a concentration of a body giving a totally different phosphorescent spectrum to the one at the first end. And it also differs chemically from old yttrium, and in a more marked manner from its brother, Gδ, at the other extremity of the fractionation. Here again its spark spectrum is perfectly identical both with old yttrium and with Gδ, and however closely I examine the three spectra in my laboratory, the whole system of lines is still identical.

Respecting the theoretical considerations involved in these results, I see two possible explanations of the facts brought forward. According to one hypothesis, research has somewhat enlarged the field lying between the indications given by ordinary coarse chemistry and the searching scrutiny of the prism. Our notions of a chemical element have expanded. Hitherto the molecule has been regarded as an aggregate of two or more atoms and no account has been taken of the architectural design on which these atoms have been joined. We may consider that the structure of a chemical element is more complicated than has hitherto been supposed. Between the molecules we are accustomed to deal with in chemical reactions and the ultimate atoms come smaller molecules or aggregates of physical atoms: these sub-molecules differ one from the other, according to the position they occupied in the yttrium edifice.

An alternative theory commends itself to chemists to the effect that the various bodies discussed above are new chemical elements differing from yttrium and samarium in basic powers and several other chemical and physical properties but not sufficiently to enable us to effect any but a slight separation. One of these bodies, Gδ gives the phosphorescent citron line and also the brilliant electric spectrum. The other seven do not give electric spectra which can be recognised in the presence of a small quantity of Gδ whilst the electric spectrum of Gδ is so sensitive that it shines out in undiminished brilliancy even when the *quantity present is extremely minute*. In the process of fractionation, Gα, Gβ, Gγ, &c. are spread out and more or less separated from one another yet the separation is imperfect at the best, and at any part there is enough Gδ to reveal its presence by the sensitive electric spark test. The arguments in favour of each theory are strong and pretty evenly balanced. The compound molecule explanation is a good working hypothesis, which I think may account for the facts, while it does not postulate the rather heroic alternative of calling into existence eight or nine new elements to explain the phenomena. However I submit it only as an hypothesis. If further research shows the new element theory is more reasonable, I shall be the first person to accept it.

Neither of these theories agrees with that of M. Lecoq de Boisbaudran, who also has worked on these earths for some time. He considers that what I have called yttrium is a true element giving a characteristic spark spectrum but not giving a phosphorescent spectrum *in vacuo*. The bodies giving the phosphorescent spectra he considers to be impurities in yttrium. These he says are two in number, and he has provisionally named them Zα and Zβ. By a method of his own, differing from mine, M. de Boisbaudran obtains uorescent spectra of these bodies, but their fluorescent bands are tremely hazy and faint, rendering identification difficult. Some of them fall near lines in the spectra of my Gβ and Gδ. At first sight it might appear that his and my spectra were due to the same bodies, but according to M. de Boisbaudran, the chemical properties of the earths producing them are widely distinct. These giving phosphorescent lines by my method occur at the yttrium extremity of the fractionation where his fluorescent bands are scarcely shown at all, whilst his fluorescent phenomena are at their maximum quite at the terbium end of the fractionation where no yttrium can be detected even by the direct spark, and where my phosphorescent lines are almost absent.

Presents, February 17, 1887.

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February 24 1887

Professor STOKES D C L President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them

The following Papers were read —

I "Problems in Mechanism regarding Trains of Pulleys and Drums of Least Weight for a given Velocity Ratio" By HENRY HENNESSY, F R S Professor of Applied Mathematics and Mechanism in the Royal College of Science, Dublin Received February 7, 1887

Eighty years have elapsed since Dr Thomas Young[*] published a theorem which has since found a place in most of the scientific treatises on mechanism This theorem states that in order to obtain a given value or velocity ratio by a train of toothed wheels and pinions of which all the pairs are equal, the ratio of the number of teeth in each wheel to the number in each pinion should be as 359 to 100, when the total number of teeth in the train is the least possible The late Professor Willis has remarked that the rule deduced from this theorem seemed not to have much practical utility, but he illustrates his remarks by referring to the trains of wheels and pinions employed in clockwork As trains of wheels, pulleys and drums, are largely employed in many machines whose arrangements greatly differ from clockwork and especially in the processes of textile manufacture, it may be interesting to examine whether there are not other conditions besides the number of teeth, which may be economised in the transformation of a movement of rotation from a moderate rate of velocity to a very high rate of velocity[†] As the number of teeth on a wheel or pinion is proportional to the circumference of the pitch-circle, it may be understood as giving a rule for deducing the ratio of the diameters of the wheels and pinions so that the sum of all their circumferences shall be a minimum. Although economy of the circumferences of wheels, speed pulleys or drums in a train may not be of much importance, is it not possible that economy of total weight of material employed may be worthy of inquiry? Reduction of weight in the parts of a machine is not merely economy of materials employed in the structure, but in the case of moving parts it involves economy of work by lessening the resistances due to friction. The following problems have arisen from such considerations, and in all of them, as well as in that studied by Young, if we call m the number of similar pairs of wheels, speed pulleys or drums, C the circumference of a large wheel, &c., and c of a small one in the same train, the velocity ratio or value of the train u will be—

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[*] 'Natural Philosophy vol 2 p 56 4to 1807. The preface to this volume is dated March 1807

[†] In some spinning machines it is said that the spindles rotate with velocities of from 6000 to 7000 turns per minute and high velocities are also often required for reels bobbins and fliers Between these rapidly rotating parts of the machines and the prime mover trains of pulleys drums or wheels are usually interposed the value of each such train depending on the required increase of velocity —[Feb 21, 1887]

$$u = (C/c)^m = (R/r)^m = x^m,$$

where x represents the ratio of the radii R and r of a large and a small wheel or pulley. In all such problems we have therefore

$$m = \log u / \log x,$$

and whether the question relates to the volume or circumference of the wheels or pulleys the usual operations of the calculus will in every case lead to a minimum.

The volumes or circumferences of pairs of pulleys or wheels with radii having the ratio x may in general be expressed in the form $Fx = a + bx + cx^2$, where a, b, and c are constants. On multiplying this by m we have—

$$V = \frac{\log u}{\log x} Fx.$$

Hence $$\frac{1}{\log u}\frac{dV}{dx} = \frac{F'x}{\log x} - \frac{Fx}{x(\log x)^2},$$

$$\frac{1}{\log u}\frac{d^2V}{dx^2}, = \frac{F''x}{\log x} - \frac{2F'x}{x(\log x)^2} + \frac{Fx}{x^2(\log x)^2} + \frac{2Fx}{x^2(\log x)^3}$$

$$= \frac{F''x}{\log x} + \frac{Fx}{x^2(\log x)^2} - \frac{2}{x\log x}\left(\frac{F'x}{\log x} - \frac{Fx}{x(\log x)^2}\right).$$

But as $$\frac{dV}{dx} = 0, \qquad \frac{F'x}{\log x} - \frac{Fx}{x(\log x)^2} = 0,$$

and $$\frac{d^2V}{dx^2} = \frac{\log u}{\log x}\left(Fx'' + \frac{Fx}{x^2 \log x}\right),$$

and from the form of $F'x$ in these problems $F''x$ and Fx are positive, therefore d^2V/dx^2 must be always a positive quantity; whence the value of x obtained in all such problems makes V a minimum.

Small pulleys carrying cords are usually made solid and approximately cylindrical, in a train of such pulleys the volumes of the large and small cylinders may be denoted by πbR^2 and πbr^2, where b is the common thickness of each cylindrical disk; the total volume of the train will therefore be—

$$V = m\pi b(R^2+r^2) = m\pi br^2(R^2/r^2+1),$$

or using the preceding notation,

$$V = \frac{\pi br^2 \log u}{\log x}(1+x^2) = K\frac{(1+x^2)}{\log x},$$

$$\frac{1}{K}\frac{dV}{dx} = \frac{2x^2\log x-(1+x^2)}{x(\log x)^2},$$

which gives $$\log x = \tfrac{1}{2}(1+x^{-2});$$

this equation is approximately satisfied by making $x = 1{\cdot}895$. Hence the ratio of the radii may be practically set down as 19 to 10 for a train of pulleys of minimum volume or least weight of material.

In drums the surface carrying the band is broad, and this surface is commonly supported by spokes which radiate from the axle, while sometimes, as in pulleys, the drum consists of a disk with a broad hoop If the thickness of the hoop and its disk are equal, a problem similar to the foregoing can be easily solved. The question is, in a series of large and small drums if all the large are equal and also all the small, required the ratio of their diameters so that the entire train shall have the least volume for a given velocity ratio. Let t be the uniform thickness of the disks and hoops of the drums, R and r the radii of a small and a large disk, b the breadth of the hoops, we shall have for the total volume of the train

$$V = m\pi[2(R+r)tb+t(R^2+r^2)],$$

when t is so small compared to R, r, and b, that quantities multiplied by t^2, &c, may be omitted.

The above may be written

$$V = \frac{\pi r^2 t\log u}{\log x}[2(x+1)b/r+x^2+1],$$

which gives, by the usual process of making $dV/dx = 0$,

$$\log x = \frac{2(x+1)b/r+x^2+1}{2x(b/r+x)}.$$

In the particular case where r is a multiple of b, or $r = nb$,

$$\log x = \frac{2(x+1)+n(x^2+1)}{2x(nx+1)};$$

and if $n = 1$,

$$\log x = \frac{1+(x+1)^2}{2x(x+1)}.$$

This equation gives $x = 2·21$ nearly, or practically a ratio of 11 to 5 for the diameters of the large and small drums in such a train as has been indicated.

Although it is manifest that the volume of a single pair of pulleys with the same velocity ratio as this train of five pairs would be considerably greater, it may be interesting to make the comparison. If R′ be the radius of the large pulley in the single pair, and as before r of the small pulley, then $R' = ur$, and the volume of the pair $V' = \pi t(u^2+1)r^2$. As before, the volume of the train is $V = \pi nt(x^2+1)r^2$.

Hence
$$\frac{V'}{V} = \frac{u^2+1}{n(x^2+1)} = \frac{x^{2n}+1}{n(x^2+1)}.$$

If $x = 1·9$ and $n = 5$, we shall have $V'/V = 26·64$, or the volume of a single pair would be more than twenty-six times the volume of a train of five pairs with the same velocity ratio.

Another solution can be easily found if a train of drums were so constructed that the volume of the spokes supporting the hoop of each drum would be half the volume of a complete disk, in this case

$$V = m\pi t\left[2(R+r)b+\tfrac{1}{2}(R^2+r^2)\right]$$
$$= \pi r^2 t\left[2(x+1)\frac{b}{r}+\tfrac{1}{2}(x^2+1)\right]\frac{\log u}{\log x},$$

and if we make $b = r$, this gives, from $dV/dx = 0$,

$$\log x = \frac{(x+2)^2+1}{2x(x+2)},$$

which is satisfied by making $x = 2·55$, or the diameters of the large drums would be to those of the small drums in the ratio of 51 to 20 in a train of least weight of drums such as here described.

If in this case b in all the drums instead of being equal to r was equal to the greater radius R, we would have evidently

$$V = m\pi r^2 t\left[2(x+1)x+\tfrac{1}{2}(x^2+1)\right]$$
$$= \tfrac{1}{2}\pi r^2 t\left[5x^2+4x+1\right]\frac{\log u}{\log x},$$

and when $dV/dx = 0$,

This will be satisfied by making x somewhat less than 1.9, so that in this case the ratio of the diameters of the drums would be a little less and very close to the ratio found for the pulleys.

In order to illustrate the foregoing problems a model of a train of pulleys and another of a train of drums made of brass were constructed by Mr. Yates. In the train of pulleys all the large ones are 1.9 inches in diameter, and all the small are 1 inch. Each of the former weighs 2.61 oz. and each of the latter 1.058 oz.; as there are five pairs their total weight is 18.340 oz., while they give a velocity ratio of $(1.9)^5 = 24.761$, or a little more than 24¾.

The train of drums consists of large ones with diameters of 2.55 inches and small of 1 inch, the hoops are in all 0.5 inch in breadth, and the spokes are half the volume of a complete disk. The weights of the large drums are each 3.386 oz., of the small 0.811 oz.

There are four pairs of drums and their total weight is 16.788 oz., or little more than 1 lb.

The velocity ratio of this train is $(51/20)^4 = 42.2825$, or a little more than 42¼.

II. "On the Relation between Tropical and Extra-Tropical Cyclones." By Hon. RALPH ABERCROMBY, F.R. Met. Soc. Communicated by R. H. SCOTT, M.A., F.R.S. Received February 7, 1887.

(Abstract.)

The conclusions as to the relation of tropical to extra tropical cyclones which the author has derived from the researches of which this paper gives an account, may be stated thus:—

All cyclones have a tendency to assume an oval form, the longer diameter may lie in any direction, but has a decided tendency to range itself nearly in a line with the direction of propagation.

The centre of the cyclone is almost invariably pressed toward one or other end of the longer diameter, but the displacement may vary during the course of the same depression.

Tropical hurricanes are usually of much smaller dimensions than extra-tropical cyclones, but the central depression is much steeper, and more pronounced in the former than in the latter.

Tropical cyclones have less tendency to split into two, or to develope secondaries, than those in higher latitudes.

A typhoon which has come from the tropics can combine with a

cyclone that has been formed outside the tropics, and form a single new, and perhaps more intense, depression

No cyclone is an isolated phenomenon, it is always related to the general distribution of pressure in the latitudes where it is generated

An area of excessive pressure, with unusually fine weather, precedes most cyclones Though the nature and origin of this high barometer is very obscure, the general character of the formation, and the weather associated with it, appear to be the same everywhere

In all latitudes a cyclone which has been generated at sea appears to have a reluctance to traverse a land area, and usually breaks up when it crosses a coast line

After the passage of a cyclone in any part of the world, there is a remarkable tendency for another to follow very soon, almost along the same track

The velocity of propagation of tropical cyclones is always small, and the average greatly less than that of European depressions

There is much less difference in the temperature and humidity before and after a tropical cyclone than in higher latitudes The quality of the heat in front is always distressing in every part of the world

The wind rotates counter clockwise round every cyclone in the Northern Hemisphere, and everywhere as an ingoing spiral The amount of incurvature for the same quadrant may vary during the course of the same cyclone, but in most tropical hurricanes the incurvature is least in front, and greatest in rear, whereas in England the greatest incurvature is usually found in the right front Some observers think that broadly speaking the incurvature of the wind decreases as we recede from the Equator

The velocity of the wind always increases as we approach the centre in a tropical cyclone, whereas in higher latitudes the strongest winds and steepest gradients are often some way from the centre In this peculiarity tropical cyclones approximate more to the type of a tornado, but the author does not think that a cyclone is only a highly developed whirlwind, as there are no transitional forms of rotating air

The general circulation of a cyclone, as shown by the motion of the clouds, appears to be the same everywhere

All over the world, unusual coloration of the sky at sunrise and sunset is observed, not only before the barometer has begun to fall at any place, but before the existence of any depression can be traced in the neighbourhood

Cirrus appears all round the cloud area of a tropical cyclone, instead of only round the front semicircle, as in higher latitudes The stripes of cirrus appear to lie more radially from the centre in the tropics, than tangentially, as indicated by the researches of Ley and Hildebrandsson in England and Sweden respectively

The general character of the cloud all round the centre is more uniform in than out of the tropics; but still the clouds in rear are always a little harder than those in front.

Everywhere the rain of a cyclone extends farther in front than in rear. Cyclone rain has a specific character, quite different from that of showers or thunderstorms, and this character is more pronounced in tropical than in extra-tropical cyclones.

Thunder or lightning is rarely observed in the heart of any cyclone, and the absence of electrical discharge is a very bad sign of the weather. Thunderstorms are, however, abundantly developed on the outskirts of tropical hurricanes

Squalls are one of the most characteristic features of a tropical cyclone, where they surround the centre on all sides; whereas in Great Britain, squalls are almost exclusively formed along that portion of the line of the trough which is south of the centre, and in the right rear of the depression. As, however, we find that the front of a British cyclone tends to form squalls when the intensity is very great, the inference seems justifiable that this feature of tropical hurricanes is simply due to their exceptional intensity

A patch of blue sky, commonly known as the "bull's-eye," is almost universal in the tropics, and apparently unknown in higher latitudes. The author's researches show that in middle latitudes the formation of a "bull's-eye" does not take place when the motion of translation is rapid; but as this blue space is not observed in British cyclones when they are moving slowly, it would appear that a certain intensity of rotation is necessary to develope this phenomenon.

The trough phenomena,—such as a squall, a sudden shift of wind, and change of cloud character and temperature, just as the barometer turns to rise, even far from the centre—which are such a prominent feature in British cyclones, have not been even noticed by many meteorologists in the tropics. The author, however, shows that there are slight indications of these phenomena everywhere; and he has collated their existence and intensity with the velocity of propagation of the whole mass of the cyclone

Every cyclone has a double symmetry. One set of phenomena, such as the oval shape, the general rotation of the wind, the cloud ring, rain area, and central blue space, are more or less related to a central point.

Another set, such as temperature, humidity, the general character of the clouds, certain shifts of wind, and a particular line of squalls, are more or less related to the front and rear of the line of the trough of a cyclone.

The author's researches show that the first set are strongly marked in the tropics, where the circulating energy of the air is great, and the velocity of propagation small; while the second set are most prominent in extra-tropical cyclones, where the rotational energy is moderate, and the translational velocity great.

The first set of characteristics may conveniently be classed together as the rotational; the second set as the translational phenomena of a cyclone.

Tropical and extra-tropical cyclones are identical in general character, but differ in certain details, due to latitude, surrounding pressure, and to the relative intensity of rotation or translation.

III. "A Thermal Telephone Transmitter." By Prof. GEORGE FORBES. Communicated by LORD RAYLEIGH, D.C.L., Sec. R.S. Received February 12, 1887.

We have had so much evidence of the sensitiveness of the Bell telephone receiver to the minutest changes of current, that we have ceased to be surprised at any transmitter which responds to the sounds of articulate speech. But, in the instrument now shown, it was so extremely unlikely that sensible variations of current could be produced with sufficient rapidity, that even now there is perhaps some interest attached to the experiment. A wooden cylinder was used closed at one end. A saw cut was made across the diameter of the closed end, making a fine slit. In the slit was stretched a platinum wire, 0·001 inch diameter and 2 inches long, with its ends connected by copper wires through the primary of an induction coil to a battery sufficiently powerful to make the platinum wire red hot. On connecting the secondary circuit with a receiving telephone in a distant room and speaking into the wooden cylinder, the words are reproduced and heard in the telephone. Each vibration of air in the slit cools the platinum wire, diminishing its electrical resistance, and increasing the electric current. The words transmitted are not quite perfect, the higher harmonics being wanting. It requires some attention to make out all the words of a sentence. A brass cylinder instead of the wooden one, and a Wollaston platinum wire of excessive fineness have been used without materially altering the clearness of the articulation. Platinum foil has hitherto given no sound of the voice. The slit in the brass instrument is made of glass to prevent the short-circuiting and destruction of the platinum wire.

Wires from one to three inches in length have been used. The longest ones are best No distinct articulation is heard if the wire be not red hot. The hotter the wire the better is the articulation. An adjustible slit was tried and the narrow slit gave the best results. Mr. Preece some years ago used the expansion and contraction of a fine platinum wire to act on a diaphragm, and so serve as a receiver The articulation seems to have been about the same in quality as when the new transmitter is used with a Bell receiver These new experiments bear out all that Mr Preece said about the rapidity of variation of temperature which can be produced in a fine platinum wire They may also be perhaps of some interest from other points of view but they are not likely to lead to any results of practical importance It is probable from the theory of the instrument that the tones are raised an octave, as is also the case in the Preece receiver

Presents, February 24, 1887

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The University

Berlin —Physikalische Gesellschaft Fortschritte der Physik im Jahre 1879 Jahrg XXXV 8vo *Berlin* 1885-86
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The University

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Utrecht — Nederlandsch Gasthuis voor Ooglijders Verslag. XXVII 8vo *Utrecht* 1886. — The Hospital.

Vienna Verein der Geographen an der Universitat Bericht uber das XI Vereinsjahr. 8vo. *Wien* 1886. — The Association

Journals.

Annales des Mines. Sér VIII Tome 10 Livr 5 8vo *Paris* 1887. — École des Mines, Paris.

Asclepiad (The) Vol. IV No 13 8vo. *London* 1887 — Dr Richardson, F.R S.

Bullettino di Bibliografia e di Storia delle Scienze Matematiche e Fisiche Marzo-Aprile, 1886 4to. *Roma* 1886 — The Prince Boncompagni.

Horological Journal. Vol. XXIX No 342 8vo. *London* 1887 — The Horological Institute

Indian Antiquary (The) Vol. XV. Part 190 4to. *Bombay* 1886. — The Editors.

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Bonney (T G) F R S On some Rocks from the neighbourhood of Assouan 8vo *Hertford* 1886, Report on the Rocks collected by H H Johnston, from the upper part of the Kilima njaro massif 8vo *London* [1886] Remarks on the Stratified and Igneous Rocks of the Valley of the Meuse in the French Ardennes 8vo [*London* 1886] The Author

Conwentz (H) Die Flora des Bernsteins Band II Die Angiospermen des Bernsteins 4to *Danzig* 1886 Prof Conwentz

Dawson (Sir W), F R S On Rhizopods in the Erian (Devonian) Period in America 8vo *Chicago* 1886 The Author

Dimmock (G) Dimmock's Special Bibliography Nos 1-3 8vo *Cambridge, Mass* 1878-79, "Psyche," a Journal of Entomology Vols III-IV Nos 101-112 Sm 4to *Cambridge, Mass* 1882-83 With Six Entomological Excerpts 8vo and 4to 1875-86 Mr G Dimmock

Jones (T R), F R S and J W Kirkby On Carboniferous Ostracoda from the Gayton Boring, Northamptonshire 8vo *Hertford* 1886,—and C D Sherborn On the Microzoa found in some Jurassic Rocks of England 8vo *Hertford* 1886,—and H B Holl Notes on the Palæozoic Bivalved Entomostraca No XXI 8vo *London* 1886 Prof T R Jones, F R S

Nikitin (S) Die Cephalopodenfauna der Jurabildungen des Gouvernements Kostroma 4to *St Petersburg* 1884, The Jurassic Formation between Rybinsk, Mologa and Myszkin [Russian] 8vo *St Petersburg* 1881, Brochure on Darwinism and Palæontology [Russian] 8vo *St Petersburg* [1882] Four Geological Excerpts 8vo and 4to The Author

Pickering (E C) Heights of the White Mountains 8vo [1887] The Author

Prince (C L) Summary of a Meteorological Journal 1885 Folio [*Crowborough* 1886] The Author

Puech (L) Memoire sur les Grottes de Sarzgel [With Photographs] 4to *Saint Affrique* 1886 The Author

Sasse (E) Das Zahlengesetz in der Völker-Reizbarkeit Statistik der neueren Geschichte von Frankreich 8vo *Brandenburg* 1877 The Author

Unwin (W C), F R S Formulæ for the Flow of Water in Pipes 4to *Manchester* 1886 The Author

Wright (T), F R S Monograph on the Lias Ammonites of the British Islands 4to *London* 1878-86 Bequeathed by the Author

March 3, 1887

Professor G G STOKES, D C L, President, in the Chair

The Presents received were laid on the table, and thanks ordered for them

In pursuance of the Statutes the names of the Candidates recommended for election into the Society were read from the Chair as follows —

Andrews, Thomas, F R S E
Atkinson, Professor Edmund, Ph D
Bottomley, James Thomson, M A
Buchanan, John Young, M A
Burbury, Samuel Hawksley M A
Buzzard, Thomas M D
Cameron, Sir Charles Alexander, M D
Carnelley, Prof Thomas, D Sc
Cash, J Theodore, M D
Corfield, Professor William Henry, M D
Davis, James William, F G S
Denton, John Bailey, M I C E
Dickinson, William Howship, M D
Douglass, Sir James Nicholas, M I C E
Ewart, Professor J Cossar, M D
Ewing, Professor J A, B Sc
Forbes, Professor George, M A
Foster, Professor Sir Balthazar Walter, F R C P
Gowers, William Richard, M D
Halliburton, William Dobinson, M D
Hinde, George Jennings, Ph D
Hyde, Henry, Major General, R E
Jervois, Sir William Francis Drummond, Lieut-General, R E
Kennedy, Professor Alexander Blackie William M I C E
Kent, William Saville
King, George, M B
Kirk, Sir John, M D
Lansdell Rev Henry D D
Latham, Peter Wallwork, M D
Lea, Arthur Sheridan D Sc
Lodge, Professor Oliver Joseph, D Sc
Lyster, George Fosbery, M I C E
Matthey, Edward F C S
Maw, George F L S
Milne Professor John, F G S
Ord, William Miller M D
Palmer, Henry Spencer, Colonel, R E
Parker, Professor T Jeffery
Pedler, Prof Alexander, F C S
Pickard Cambridge, Rev Octavius, M A
Pickering, Professor Spencer Umfreville, M A
Poynting, Professor John Henry, B Sc
Pritchard, Urban, M D
Ramsay, Professor William, Ph D
Seebohm Henry, F L S
Smith, Willoughby
Snelus, George James, F C S
Sollas, Professor William Johnson, D Sc

Stevenson, Thomas, M D
Teale, Thomas Pridgin, F R C S
Tenison-Woods, Rev Julian E, M A
Thin, George, M D
Tidy, Professor Charles Meymott, M B
Todd, Charles, M A
Tomlinson, Herbert, B A
Topley, William, F G S
Ulrich, Professor George Henry Frederic, F G S
Walsingham, Thomas, Lord
Whitaker, William, B A
Yeo, Professor Gerald F, M D

The following Papers were read —

I "Preliminary Note on a Balanoglossus Larva from the Bahamas" By W F R WELDON, M A, Fellow of St John's College, Cambridge Communicated by Prof M FOSTER, Sec R S Received February 15, 1887

In October last, during a visit to the Island of Bemini, on the western edge of the Bahama bank, an organism was constantly found in the tow net which closely resembled the larva of Balanoglossus recently described by Bateson [*]

In the youngest stage observed, this creature has an elongated cylindrical body (about 0 8 mm long by 0 4 mm broad) with rounded ends At the anterior extremity are two eye spots while near the posterior is a large and powerful ring of cilia An anterior region is separated from the rest by a deep transverse groove, more than this cannot be made out by examination of entire specimens [†] A little later, a second shallower groove appears behind the first, marking off a smaller middle region of the body from the larger anterior and posterior divisions An idea of the shape of the body, just before the appearance of the second transverse groove, may be gathered from the nearly median longitudinal section (fig 1) In this section the mouth is seen to lie in the first transverse groove, on the ventral side of the body, it leads to a well developed alimentary canal, ending in a median posterior anus, not seen in the figure On each side of the alimentary canal lie two sections of body cavity, the first (fig 1, II) can hardly be spoken of as a cavity, its lumen being never conspicuous, and often obliterated, behind this is a well developed posterior cavity (III) The body cavities of the two sides are separated

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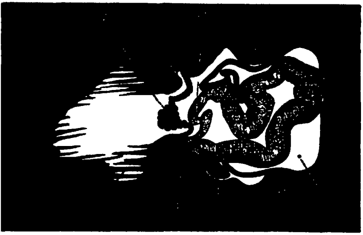
[*] 'Roy Soc Proc,' vol 38 1885 p 23; and Quart Journ Microsc Sci 1885 and 1886

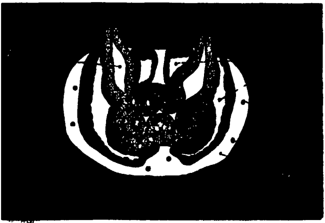
[†] I regret that my observations on the living larva are most imperfect Owing to my want of experience in protecting delicate organisms after capture from a tropical sun I was frequently obliged to preserve the material obtained in an open boat, where microscopic work was impossible

Fig 1



from one another in the middle line by a considerable interval and the posterior pair especially leave a considerable part of the blastocœl unoccupied (fig 2 *Bl*) The anterior nearly solid body cavities

Fig



are separated dorsally by a forward process of the posterior cavities, so that a section so near the middle line as that drawn in fig 1 does not cut them dorsally at all

The anterior region of the body in front of the great transverse groove is occupied by a single unpaired body cavity (fig. 1, I), which obliterates the blastocœl This cavity is traversed by a number of longitudinal "mesenchymatous" muscles (*Mc.*), and carries on its floor a glandular organ (*Gl*), near which opens, asymmetrically, a short canal, which runs to the middle dorsal line, where it communicates with the exterior by a pore (*P.*). Immediately beneath the gland is a forwardly directed diverticulum of the gut (*Ch.*).

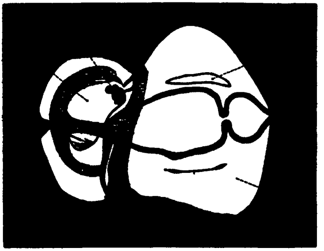
It seems impossible to avoid identifying these structures with the proboscis gland and pore of Balanoglossus, and the subjacent "notochord"; while the anterior paired body cavity represents the collar cavity, the posterior the trunk cavity, of the normal Balanoglossus larva. A confirmation of this view is given by the appearance, at a slightly later date, of a single pair of rudimentary gills (fig 2, *Br.*).

The ectoderm of the larva, up to the point at which the gills appear, is very thick (figs. 1 and 2), and contains many mucus and other glandular cells; while beneath the eye-spots is a well-developed "Scheitelplatte," and beneath the general ectoderm a well-formed embryonic nervous system, the details of which I reserve for a later paper

Just after the development of the gill-slits, there appears to be much variation in the conduct of the larvæ obtained; some exhibit indications of a normal development, the majority, however, begin from this point to undergo a gradual process of degeneration, accompanied by considerable increase in size.

The shape of the most degenerate larva obtained may be gathered from the nearly median longitudinal section (fig. 3); where the collar

FIG 3.



groove is seen to have disappeared altogether, while that behind the proboscis has become much shallower. The external appearance is complicated by the formation, on each side of the proboscis, of a ⋒-shaped groove, the middle limb of which communicates with the post-proboscidean groove, while the margins of all the limbs are provided with short, broad tentacles. The arrangement of this groove is indicated by the shading between dotted lines in fig. 3.

Sections show that the mesoblastic organs have undergone considerable reduction, both relative and, in some cases, absolute. The proboscis cavity is smaller (fig. 3, I); its walls are thinner, and its muscles fewer. The notochord beneath it has quite disappeared; the collar cavities have disappeared; while the trunk cavities are small and thin-walled (fig. 3, III). No trace of gill-pouches remains. The ectoderm is much thinner, and contains hardly a trace of any nervous structure except the much diminished "Scheitelplatte" (*Sch.*), on which the eye-spots still persist.

In connexion with this degeneration of the tissues, it may be noticed that many cells, possibly phagocytes, are present in the blastocœl at earlier stages (fig 2, *Ph. ?*).

I was not able during my stay at Bemini to follow this creature further; but at Nassau, New Providence, in the middle of the Bahama bank, I observed, during four months, a similar series of changes in a much larger larva. This larva was first obtained at a period just before the development of the tentacular apparatus, and after the disappearance of the collar groove (if this ever existed). The collar and trunk cavities were both well developed, and the proboscis cavity, with its gland and pore, was as in the youngest Bemini forms. Eye-spots were present, and there was a well-developed cutaneous nerve plexus. In this form degradation was followed to a much fuller extent, till the ectoderm was (except on the well-developed tentacles and beneath the cilia) a mere flattened epithelium; the trunk cavity was a minute solid rod beneath the ciliated ring; the collar cavity disappeared, and the reduction of the proboscis cavity was carried much further than in the Bemini form.

I hope to publish a fuller account of both forms in a subsequent paper. In the meantime, it is submitted that there is fair ground for the belief that the organisms described are Balanoglossus larvæ, which from some cause or other have been unable to develop adult characters, and have therefore varied. Independent evidence shows that a probable cause may be the compulsory shifting of the larvæ into deep water by the joint action of currents and winds.*

If this be admitted, four things follow:—*First*, that, at least in some cases, the transmission by a larva of hereditary changes is only, possible on the application of the stimuli afforded by particular surroundings; *secondly*, that some larvæ, in the absence of these stimuli, but in conditions otherwise favourable, are highly variable; *thirdly*, that the variations produced by a given change in the environment may be of an uniform and definite character; and lastly, that these changes may result, not in the modification of ancestral organs, but in the hypertrophy of those which are purely larval.

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* These larvæ were practically all caught outside the 100-fathom line.

The last of these considerations leads to the hope that a further investigation of similar cases may afford a criterion by which to interpret larval histories in general.

EXPLANATION OF THE FIGURES

Fig. 1.—Lateral longitudinal section (nearly median) through a young Bemini larva, just before the appearance of the collar-fold.

Fig. 2.—Transverse section through the trunk of a Bemini larva, at the time of the greatest development of the gill-pouches

Fig. 3.—Nearly median longitudinal section through a degenerate Bemini larva. The arrangement of the tentaculiferous grooves is indicated by shading within the dotted lines.

Reference Letters.—*An.*, anus; *Bl.*, blastocœl; *Br*, branchial pouch; *Ch.*, "notochord" of Bateson; *Ci.*, cilia; *Gl.*, proboscis gland; *M.*, mouth; *Mc.*, "mesenchym" of proboscis cavity; *P.*, proboscis pore; *Ph.?*, cells of blastocœl, possibly phagocytes; *Sch.*, "scheitelplatte"; I, II, III, body cavities of proboscis, collar, and trunk respectively.

II. "Studies of some New Micro-organisms obtained from Air." By G. C. Frankland, and Percy F. Frankland, Ph.D., B.Sc. (Lond.), F.C.S., F.I.C. Communicated by E. Ray Lankester, M.A., F.R.S., Professor of Zoology, University College, London. Received February 15, 1887.

(Abstract.)

In previous communications to the Royal Society by one of the authors,* details have been given of a number of experiments on the presence of micro-organisms in the atmosphere. In these investigations a solid culture medium was employed, which not only greatly facilitated their enumeration, but also presented them in an *isolated* condition. In this manner the authors have met with a number of different varieties of aerial micro-organisms, which have hitherto remained either unknown or undescribed They have therefore undertaken the characterisation of a number of these organisms by growing them in various cultivating media and observing the different appearances which they subsequently exhibit by studying them microscopically in stained and unstained preparations, and by cultivating them on gelatine plates, and describing the colonies to which they give rise They have likewise made a number of drawings to illustrate the appearances which they present under the various examinations to which they have submitted them To further facilitate their identification the authors have provisionally given them names, by which they have endeavoured to represent some of their most striking individualities

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* 1. "The Distribution of Micro-organisms in Air," 'Roy. Soc. Proc.,' vol. 40, p. 509; 2. "A New Method for the Quantitative Estimation of the Micro-organisms present in the Atmosphere," *ibid.*, vol. 41, p. 443; 3. "Further Experiments on the Distribution of Micro-organisms in Air by Hesse's method," *ibid.*, p. 446.

The authors venture to hope that by thus characterising some of the organisms most prevalent in the atmosphere, they may prove of assistance in those investigations which have for their object the study of the particular physiological changes which are brought about by specific micro organisms

The following is a list of the micro-organisms described —

Micrococcus carnicolor
„ albus
„ gigas
„ chryseus
„ candicans
Streptococcus liquefaciens
Sarcina liquefaciens
Bacillus aurescens siccus
„ aureus
„ citreus

Bacillus plicatus
„ chlorinus
„ polymorphus
„ profusus
„ pestifer vermicularis
„ subtilis minor
„ subtilis cereus
Saccharomyces rosaceus
„ liquefaciens
Mycelium fuscum

In addition to these varieties a description has been given for the sake of comparison of some aërial micro-organisms which were obtained by one of the authors from Dr Koch's laboratory in Berlin These are—

Micrococcus rosaceus
Sarcina lutea
„ aurantiaca

Bacillus subtilis
„ (Micrococcus) prodigiosus

III. "On the Limiting Distance of Speech by Telephone." By William Henry Preece, F.R.S. Received February 17, 1887.

The law that determines the distance to which speaking by telephone on land lines is possible, is just the same as that which determines the number of currents which can be transmitted through a submarine cable in a second. The experimental evidence upon which this law is based was carried out in 1853 by Mr. Latimer Clark (whose assistant I then was). The experiments were made by me in the presence of Faraday; many were his own; he made them the subject of a Friday evening discourse at the Royal Institution, January 20, 1854, and they are published in his 'Researches' (vol. 3, p. 508). They received full mathematical development by Sir William Thomson in 1855 ('Roy. Soc. Proc.,' May 24), who determined the law, the accuracy of which was proved by Fleeming Jenkin and by Cromwell Varley, and the 110,000 miles of cable that now lie at the bottom of the ocean afford a constant proof in their daily working.

Hockin reduced Thomson's law to the following series:—

$$x = C(1-2\{(\tfrac{1}{4})^{t/a}-(\tfrac{1}{4})^{4t/a}+(\tfrac{1}{4})^{9t/a}-(\tfrac{1}{4})^{16t/a}+\&c.\}),$$

which allows it to be expressed by the following curve (1):—

Now a is a time-constant dependent on the conditions of the circuit, invariable for the same uniform circuit but differing for different circuits. It represents the time that elapses from the instant contact is made at the sending end to the instant that the current begins to appear at the receiving end. It is given by the following equation:—

$$a = Bkrl^2,$$

B being a constant dependent principally on the units used; k the inductive capacity per unit length (mile or knot); r the resistance per unit length, and l the length in miles or knots.

a, therefore, limits the number of vibrations per second that can be sent through any circuit. If a be 0·196 second, as it was in the French Atlantic cable of 1869,* 2584 knots long, then it is impossible to send 5·1 currents per second through that cable; but it would be possible to send 5 or 2½ complete reversals per second. Moreover, as the number of reversals varies inversely with the square of the length, it shows that such a cable if of 100 miles length would allow 1562 reversals to pass through it. It is necessary to remark that these expressions involve no mention of E M.F., or of current, and therefore the number of reversals which can be produced at the end of a wire is quite independent of the impressed E.M F., and therefore of the strength of the current. But the number of reversals is dependent upon the sensitiveness of the apparatus used to receive the currents, for if we use an instrument which will respond to a current indicated by the full line, we get two currents per second; and if we use an instrument which will respond only to the dotted line, we get only one current in two seconds, which is about the current used with delicate relays in this country. This is why such discordant results are obtained by different observers who attempt to measure the velocity of currents of electricity. It is also why the telephone is such an admirable instrument for research—for it is sensitive to the least increment or decrement of current.

Before proceeding with this inquiry it was necessary to determine very accurately the inductive capacity of overhead and underground wires. This was done with great care on very dry days in different parts of the country by means of a Thomson mirror galvanometer and a standard condenser.

The results come out as follows:—

	Capacity per mile microfarads.	Resistance per mile B. A. ohms.
No. 7½ iron wire	0·0168	12·0
No. 12½ copper wire	0 0124	5 7
Gutta-percha-covered wire in iron pipes	0·2500	23·0
Gutta-percha-covered wire in cables..........	0·2900	10·25

The capacity can be calculated from the following formula (also due to Sir William Thomson):—

$$K = \frac{l}{2 \log (4h/d)},$$

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* Fleeming Jenkin, 'Electricity and Magnetism,' p. 331.

where d is the diameter of the wire and h the height above the ground Taking h at 500 cm and d at 0 243 cm, the capacity comes out for 12½ wire at 0 0113 instead of 0 0124 microfarad per mile. The cause of this difference is referred to further on

It then became necessary to determine the speed of the current through wires of different lengths, resistances, and capacities

A multiplex distributor—such as we are now using in the Post Office—enables this to be done with great accuracy At each station a circle is broken up into 162 sections, and an arm in connexion with the line wire carrying a brush sweeps over these sections and makes contact This arm can be made to rotate at any speed If it makes three revolutions per second it will make the duration of each contact $\frac{1}{486}$th of a second Now the two distributors are kept running in absolute synchronism, so that the brush at each station always rests simultaneously on corresponding sections The sections at one station can be placed in contact with the battery, and those at the other station with a galvanometer or other sensitive apparatus If the current traversed the wire instantaneously then it would appear on the same section at the same time on the galvanometer, but the current is always retarded, and the amount of retardation or the value of a can be measured and the curve of arrival and cessation drawn by watching the indications of the galvanometer on each succeeding section

The following table summarises a large number of experiments that have been made in different parts of the country, and on different lines, to determine by observation the connexion that exists between speed of current distance spoken through, resistance, and capacity The maximum current, or the crest of the wave, was observed This was equivalent to 0 003 ampère

It will be seen that the limiting distance through which it is possible to speak varies inversely with the speed of the current, and that the speed of the current varies inversely with the product of the total resistance and the total capacity of the circuit Hence we can say that the number of reversals that it is possible to send through any circuit varies inversely with the product of the total resistance (R) and the total capacity (K) or the limiting distance

$$S = KR \times \text{constant} \quad . \qquad (1)$$

This is only another form of Thomson's law for $K = lk$, and $R = lr$, and

$$\therefore\ S = krl^2 \times \text{constant}$$

It is seen that when the speed of the working current was

0 001 speaking was perfect,
0 002′ speaking was good,
0 003′ speaking was fair,
0 004′ speaking was difficult

Table showing the Speed of Current through Wires

Wire	Route	Length in miles	Total resistance × Total capacity	Speed of current in seconds	Telephonic notes.
Nos 142 and 144, Denham and Atherstone	New West Coast { Open Covered	90 2 5	2247 0	0 0007 calculated	Gower Bell Telephone Speaking good
London and Denham	Underground	21	2963 5	0 0009 calculated	Ditto ditto
Nos 142 and 144, Denham and Stafford	New West Coast { Open Covered	119 5	4715 7	0 0015 calculated	Gower Bell Telephone Speaking fairly good
South Wales	{ Open Covered	79 1 25	5680 0	0 0016 calculated	Gower Bell Telephones working satisfactorily
Newcastle Telephone system Warkworth to Stockton P O	*Viâ* Newcastle Sunderland and West Hartlepool { Open Covered	63 831 12 039	5817 6	0 0016 calculated	Gower Bell Telephones o intermediate electro magnets of 1000 ohms resistance in derived circuit Speaking satisfactory
Nos 142 and 144, London and Towcester	New West Coast { Open Covered	50 21	7341 3	0 00234 calculated	Gower Bell Telephone Speaking weak but just able to hold a conversation
Nos 142 and 144 Denham and Nantwich	Ditto { Open Covered.	146 7	7612 1	0 0034 calculated	Gower Bell Telephone Speaking weak but just able to carry on a conversation

Table showing the Speed of Current through Wire—*continued*.

Wire.	Route.		Length in miles.	Total resistance × Total capacity.	Speed of current in seconds.	Telephonic notes.
London to Denham (with additional loop between Denham and Hanwell)..	Underground..........		39	10221·0	0·0032 calculated.	Gower-Bell Telephone. Speaking good, but approaching the point of difficulty.
Nos. 142 and 144, Denham to Warrington	New West Coast..	Open...... Covered....	172 8	10899·16	0·0038 calculated	Only just able to speak. Impossible to carry on conversation.
Nos. 142 and 144, London to Atherstone..........	Ditto.	Open...... Covered ...	90 23·5	12511·2	0 004 calculated.	Only just possible to speak.
New Irish Cable	Cable....	Open...... Covered....	25 62 9	12587·4	0·00412 observed.	Berliner's Telephone. Only just able to talk.
Newcastle district........	....	Open...... Covered.. .	30 30	12640·0	0·00412 calculated	Requires very good telephones and very good voices.
Copper. London to Nevin (land lines for the new Irish cable)	*Viâ* Worcester and Shrewsbury..	Open .. Covered	251·18 18 5	15771·5	*0·0035 observed.	Berliner's Telephone. Some voices good.
No. 11, London Stock Exchange to Liverpool Stock Exchange (Iron wire, No. 8 gauge)......	*Viâ* London and N.W. Railway ..	Open... Covered.	199·014 7·243	16417·8	0·605 observed.	Telephone not tried.

[illegible].—The speed is [illegible] d from the formula $t = 32$ [illegible]

* The [illegible] for copper is 22×10^{-6}.

If we put equation (1) into this form,

$$A = krx^2, \tag{2}$$

and give to A the following values —

Copper (overhead)	15,000
Cables and underground	12,000
Iron (overhead)	10,000

we can find the limiting distance we can speak with any wire, for

$$x^2 = A/kr.$$

Take copper, whose constant is 15,000, and a wire whose resistance is 1^{ω} per mile, and capacity 0 0124 per mile, then—

$$x^2 = \frac{15000}{0\,0124},$$

$$x = 1100,$$

which is the limit of speaking upon such a wire

The wire used between Paris and Brussels has a resistance of 2 4 ohms and a capacity of about 0 012 microfarad per kilometre and as that distance is only about 200 miles the speaking must be excellent Moreover, there is reason to believe from the difference between observation and calculation, that the static capacity on Continental and American lines is less than that of English lines, owing to the use of earth wires on all poles in England, and therefore the distance would be greater

Take an Atlantic cable—

$$x^2 = \frac{12000}{3\times 0\,43},$$

$$x = 96$$

Now I had found in 1878* that it was just possible to speak through 100 miles of such a cable—a very close agreement

Moreover, by the law of the squares, 100 miles of an Atlantic cable ought to transmit 1562 reversals, if 2584 miles transmit 2½ and this is probably the average number of sonorous vibrations imparted by the human voice, when hearing by telephone begins to get difficult by the loss of the higher partials and overtones

There is another interesting consequence of Thomson's law which comes out of these experiments, and that is, whether the line be a single wire completed by the earth, or a double wire making a metallic circuit, the rate of speed between the two ends is exactly the same, and therefore the distance we can speak through is just the same whether we use a single or double wire circuit This is owing to the fact that though in the latter case we double the total resistance, we halve the total capacity, and therefore the product remains the same

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* 'Phil Mag,' April 1878

The difference between copper and iron is clearly due to self-induction or to the electromagnetic inertia of the latter and the difference between copper overground and copper underground is due to the facility that the leakage of insulators offers to the rapid discharge to earth at innumerable points of the static charge, which in gutta percha covered wire can find an exit only at the ends

It is also evident that there is no difficulty in working telephones through underground wires even though they attain 50 miles in length, and in fact it would be better to work underground with proper copper wire from London to Brighton than to use iron wires along the railway telegraph poles owing to the absence of external disturbances in the former case

The limit of working of different insulated wires is easily obtained by equation (2) and the following table gives that information for different gutta percha covered wires

No	k	r	Limit of speech
20 11	0 270 m f	45 00 ohms	32 miles
18 7½	0 250	23 00	46
16 4	0 240	13 00	62
107 lbs 150 lbs	0 290	10 25	64 ,,

N B —The top number indicates the gauge of wire and the lower n
of the gutta percha.

IV "The Etiology of Scarlet Fever" By E Klein, M D F R S, Lecturer on General Anatomy and Physiology at the Medical School of St Bartholomew's Hospital London Received February 23, 1887

The investigation the results of which I now record was commenced at the end of December 1885 It arose out of an inquiry into the prevalence of scarlatina in different quarters of London, undertaken by the Medical Department of the Local Government Board as a part of its business of investigating local epidemics That inquiry had demonstrated milk from a farm at Hendon as the cause of the scarlatina, and had adduced strong circumstantial evidence that the scarlatina had been distributed, not in the whole, but in certain sections of the Hendon milk, and further that the ability of the sections of milk service to convey the disease had been related to a malady affecting particular cows This evidence against particular cows at the Hendon farm could not and did not aspire at furnishing direct and definite proof of the connexion of this cow disease with scarlet fever of man, for the inductive methods usually employed by the Medical Department of the Local Government Board when applied to inquiries about epidemic spread of scarlatina can for obvious reasons yield but circumstantial evidence As on various former occasions, so also on this, the Medical Department sought to put the above conclusions to the test of scientific experiment This task was delegated to me by the Board The first part of this work has been published in the recently issued volume of the Reports of the Medical Officer of the Local Government Board for 1885-1886 I have therein shown that the suspected cows from the Hendon farm that had been made the object of special study showed besides a skin disease—consisting in ulcers on the udder and teats, and in sores and scurfy patches and loss of hair in different parts of the skin—also a general disease of the viscera, notably the lungs, liver spleen, and kidney, which resembled the disease of these organs in acute cases of human scarlatina I have further shown that the diseased tissues of the ulcers on the teats and udder produced on inoculation into the skin of calves a similar local disease, which in its incubation and general anatomical characters proved identical with the ulceration of the cow and further, that from the ulcers of the cow a species of micrococcus was isolated by cultivation in artificial nutritive media, which micro organism in its mode of growth on nutritive gelatine, on Agar-Agar mixture, on blood serum, in broth and in milk, proved very peculiar and different from other species of micrococci hitherto examined With such cultivation of the micrococcus I have produced by subcutaneous inoculation in calves a disease which in its cutaneous and visceral lesions (lung liver, spleen, and kidney) bears a very close resemblance both to the disease that was observed in the Hendon cows as well as to human scarlatina

The second part of the work, carried out during 1886-1887 for the Medical Department, had for its object to investigate whether or no the disease, human scarlatina, is associated with the identical micrococcus, and whether this, if obtainable from the human subject, is capable of producing in the bovine species the same disease as was observed in the Hendon cows and in the calves experimented upon from the latter source. The definite and clear proof that this is really the case has now been obtained, and the evidence I now bring to the notice of the Royal Society.

On examining acute cases of human scarlatina—for which opportunity I owe great thanks to Dr. Sweeting, the Medical Superintendent of the Fulham Fever Hospital—I soon ascertained the fact that there is present in the blood of the general circulation a species of micrococcus, which on cultivation in nutritive gelatine, Agar-Agar mixture, blood serum, and other media, proved to be in every respect identical with that obtained from the Hendon cows. Out of eleven acute cases of scarlet fever examined in this direction, four yielded positive results: three were acute cases between the third and sixth day of illness with high fever temperature, and the fourth was a case of death from scarlatina on the sixth day. In all these four cases several drops of blood were used, after the customary methods and under the required precautions for establishing cultivations in a series of tubes containing sterilised nutritive gelatine, and generally only a very small number of these tubes revealed after an incubation of several days one or two colonies of the micrococcus. This shows that the micrococci were present in the blood in but small numbers.

Having ascertained the identity in morphological and cultural respects of the micrococcus of the blood of human scarlatina with the organism obtained from the Hendon cows, the action of the cultivations of both these sets of micrococci was then tested on animals and the results compared. It was found that mice—wild mice better than tame ones—on inoculation as well as feeding, became affected in exactly the same manner, no matter whether the one set of cultivations or the other was used. The great majority of these animals died after between seven and twenty days; the post-mortem examination revealed great congestion of the lungs, amounting in some cases to consolidation of portions of the organ, congestion of the liver, congestion and swelling of the spleen, great congestion and general disease of the cortical part of the kidney. From the blood of these animals, taken directly from the heart, cultivations were established in nutritive gelatine, and hereby the existence of the same species of micrococci was revealed; they possessed all those special characters distinguishing the cultivations of the micrococcus of the Hendon cows and of the human scarlatina.

In the third and concluding section of the work, cultivations of the micrococcus of two cases of human scarlatina were used for infecting calves; two calves were inoculated and two were fed from each set of cultivations. All eight animals developed disease, both cutaneous and visceral, identical to that produced in the calves that had been last year infected with the micrococcus from the Hendon cows.

From the heart's blood of calves thus infected from human scarlatina the same micrococcus was recovered by cultivation, possessing all the characters shown by the cultures of the micrococcus of the Hendon cows, and of the cases of human scarlatina

It must be evident from these observations that the danger of scarlatinal infection from the disease in the cow is real, and that towards the study and careful supervision of this cow disease all efforts ought to be directed in order to check the spread of scarlet fever in man It is also obvious that in the agricultural interest alone investigations of this cow disease are greatly called for

Presents *March* 3, 1887

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March 10, 1887.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Note on Induction Coils or 'Transformers.'" By JOHN HOPKINSON, M.A., D.Sc., F.R.S. Received February 17, 1887.

The transformers considered are those having a continuous iron magnetic circuit of uniform section.[*]

Let A be area of section of the core,

- m and n the number of convolutions of the primary and secondary coils respectively,
- R, r, and ρ their resistances, ρ being the resistance of the secondary external to the transformer,
- x and y currents in the two coils,
- a induction per square centimetre,
- α the magnetic force,
- l the length of the magnetic circuit,
- $E = B \sin 2\pi(t/T)$, the difference of potentials between the extremities of the primary,
- T being the periodic time.

We have

(1.) $4\pi(mx+ny) = l\alpha$;

(2.) $E = Rx - mA\dot{a}$;

(3.) $0 = (r+\rho)y - nA\dot{a}$;

from (2) and (3),

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[*] For a discussion of transformers in which there is a considerable gap in the magnetic circuit, see Ferraris, 'Torino, Accad. Sci. Mem.,' vol. 37, 1885; Hopkinson, "On the Theory of Alternate Currents," 'Telegr. Engin. Journ.,' vol. 13, 1884, p 496.

$$(4.)\ n\mathrm{E} = n\mathrm{R}x - m(r+\rho)y\,;$$

substituting from (1),

$$(5.)\ x\{n^2\mathrm{R}+m^2(r+\rho)\} = n^2\mathrm{E}+(l\alpha/4\pi)m(r+\rho),$$

$$(6.)\ y\{n^2\mathrm{R}+m^2(r+\rho)\} = -nm\mathrm{E}+(l\alpha/4\pi)n\mathrm{R}\,;$$

$$(7.)\ \mathrm{A}\dot{a} = -\frac{(r+\rho)m\mathrm{E}}{n^2\mathrm{R}+m^2(r+\rho)} + \frac{l\alpha\mathrm{R}(r+\rho)}{4\pi\{n^2\mathrm{R}+m^2(r+\rho)\}}.$$

We may now advantageously make a first approximation, neglect $l\alpha$ in comparison with $4\pi m\alpha$, that is, assume the permeability to be very large, we have

$$(8.)\ \mathrm{A}\dot{a} = -\frac{(r+\rho)m\mathrm{B}\sin(2\pi t/\mathrm{T})}{n^2\mathrm{R}+m^2(r+\rho)}\,;$$

$$(9.)\ \mathrm{A}a = \frac{(r+\rho)m\mathrm{B}\cos(2\pi t/\mathrm{T})}{\{n^2\mathrm{R}+m^2(r+\rho)\}\,.\,2\pi t/\mathrm{T}}.$$

For practical purposes these equations are really sufficient.

We see firstly that the transformer transforms the potential in the ratio n/m, and adds to the external resistance of the secondary circuit ρ a resistance $(n^2\mathrm{R}/m^2)+r$. This at once gives us the variation of potential caused by varying the number of lamps used. The phase of the secondary current is exactly opposite to that of the primary.

In designing a transformer it is particularly necessary to take note of equation (9), for the assumption is that a is limited so that $l\alpha$ may be neglected. The greatest value of a is $\mathrm{B}/\{(2\pi/\mathrm{T})m\mathrm{A}\}$, and this must not exceed a chosen value. We observe that B varies as the number of reversals of the primary current per unit of time.

But this first approximation, though enough for practical work, gives no account of what happens when transformers are worked so that the iron is nearly saturated, or how energy is wasted in the iron core by the continual reversal of its magnetism. The amount of such waste is easily estimated from Ewing's results when the extreme value of a is known, but it is more instructive to proceed to a second approximation, and see how the magnetic properties of the iron affect the value and phase of x and y. We shall as a second approximation substitute in equations (5) (6), (7) values of α deduced from the value of a furnished by the first approximation in equation (9).

In the accompanying diagram Ox represents α, Oy represents a, and Oz the time, t.

The curves ABCD represent the relations of a and α. EFG the induction a as a function of the time, and HIK the deduced relation between α and t. We may substitute the values of α obtained

FIG 1

from this curve in equations (5) and (6), and so obtain the values of x and y to a higher degree of approximation If the values of a were expressed by Fourier's theorem in terms of the time, we should find that the action of the iron core introduced into the expression for x and y, in addition to a term in cos $(2\pi t/\mathrm{T})$ which would occur if a and α were proportional, terms in sin $(2\pi t/\mathrm{T})$ and terms in sines and cosines of multiples of $2\pi t/\mathrm{T}$ It is through the term in sin $(2\pi t/\mathrm{T})$ that the loss of energy by hysteresis comes in

A particular case, in which to stay at a first approximation would be very misleading, is worthy of note Let an attempt be made to ascertain the highest possible values of a by using upon a transformer a very large primary current, and measuring the consequent mean square of potential in the secondary circuit by means of an electrometer, by the heating of a conductor, or other such device The value of a will be related to the time somewhat as indicated by ABCDEFG in fig 2, for simplicity assume it be as in fig 3, the

FIG 2.

FIG. 8.

resulting relations of potential in the secondary and the time will be indicated by the dotted line HIJKOLMNPQ. The mean square observed will be proportional to $ML \sqrt{LP}$, but $ML \cdot LP$ is proportional to EL, hence the potential observed will vary inversely as $\sqrt{LP}$ even though the maximum induction remain constant. If then the maximum induction be deduced on the assumption that the induction is a simple harmonic function of the time, results may readily be obtained vastly in excess of the truth.

II. "Note on the Theory of the Alternate Current Dynamo." By JOHN HOPKINSON, M.A., D.Sc., F.R.S. Received February 17, 1887.

According to the accepted theory of the alternate current dynamo, the equation of electric current in the armature is $\gamma y + Ry =$ periodic function of t, where γ is a constant coefficient of self induction. This equation is not strictly true, inasmuch as γ is not in general constant,* but it is a most useful approximation. My present purpose is to indicate how the values of γ and of the periodic function representing the electromotive force can be calculated in a machine of given configuration.

To fix ideas we will suppose the machine considered to have its magnet cores arranged parallel to the axis of rotation, that the cores are of uniform section, also that the armature bobbins have iron cores, so that we regard all the lines of induction as passing either through an armature coil, or else between adjacent poles entirely outside the armature. The sketch shows a development of the machine considered. The iron is supposed to be so arranged that the currents induced therein may be neglected We further suppose for simplicity that the line integral of magnetic force within the armature core may be neglected

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* "On the Theory of Alternating Currents," 'Telegr. Engin. Journ.,' vol. 13, 1884, p. 496.

Let A_1 be the effective area of the space between the pole piece and armature core when the cores are in line, l_1 the distance from iron to iron

Let A_2 be the section of magnet core, l_2 the effective length of a pair of magnet limbs, so that l_2 may be regarded as the length of the lines of force as measured from one pole face to the next

Let m be the number of convolutions in a pair of magnet limbs, and

n, the convolutions in one armature section,

T, the periodic time

The time is measured from an epoch when the armature coil we shall consider is in a symmetrical position in a field which we shall regard as positive

x and y are the currents in the magnet and armature coils, the positive direction being that which produces the positive field at time zero

At time t the armature coil considered has area A_1,

$$= b_0 + b_1 \cos(2\pi t/\mathrm{T}) + b_2 \cos(4\pi t/\mathrm{T}) + \&c$$

in a positive field, and area A_1,

$$= b_0 - b_1 \cos(2\pi t/\mathrm{T}) + b_2 \cos(4\pi t/\mathrm{T}) - \&c$$

in a negative field, where

$$b_0 + b_1 + b_2 + \quad = A_1,$$

and

$$b_0 - b_1 + b_2 + \quad . \quad = 0$$

The coefficients b_0, b_1, &c, are deducible by Fourier's theorem from a drawing of the machine under consideration

Let I be the total induction in the magnet core, and let at time t I be distributed into I through A_1, I through A_1' and I as a waste field to the neighbouring poles

The line integral of magnetic force from the pole to either adjacent pole is I $/k$, where k is a constant

We have first to determine I, I, I, in terms of x and y

Take the line integral of magnetic force in three ways through the magnets, and respectively through area A_1, through area A_1, and across between the adjacent poles—

$$l_2 f\left(\frac{I}{A_2}\right)+2l_1\frac{I}{A_1} = 4\pi mx+4\pi ny,$$

$$l_2 f\left(\frac{I}{A_2}\right)+2l_1\frac{I}{A_1} = 4\pi mx-4\pi ny$$

$$l_2 f\left(\frac{I}{A_2}\right)+\frac{I}{k} = 4\pi mx$$

whence $$I+I+I = I$$

$$= \left(\frac{A_1+A_1}{2l_1}+k\right)\left(4\pi mx-l_2 f\left(\frac{I}{A_2}\right)\right)+\frac{A_1-A}{2l_1}\ 4\pi ny$$

When t x, and y are given this would suffice to determine I by means of the known properties of the material of the magnets as represented by the function f We will however consider two extreme cases between which other cases will lie

First —Suppose that the intensity of induction in the magnet cores is small, so that $l_2 f(I/A_2)$ may be neglected, the iron being very far from saturation We have—

$$I = \frac{4\pi}{2l_1}\left\{m(A_1-A_1\ )x+n(A_1+A_1\ )y\right\}$$

$$= \frac{4\pi}{l_1}\left\{m\left(b_1\cos\frac{2\pi t}{T}+b_3\cos\frac{6\pi t}{T}+\qquad\right)x\right.$$

$$\left.+n\left(b_0+b_2\cos\frac{4\pi t}{T}+\qquad\right)y\right\}$$

We see that the coefficient of self induction γ in general contains terms in cos $(4\pi t/T)$

Second —In actual work it would be nearer the truth to suppose that the magnetising current x is so great that the induction I may be regarded as constant and the quantity $l_2 f(I/A_2)$ as considerable But as small changes in I imply very great changes in $l_2 f(I/A_2)$, its value cannot be regarded as known We have then—

$$2l_1\frac{I'}{A_1} = \frac{2l_1}{A_1+A_1+2kl_1}\left(I-\frac{A_1-A_1}{2l_1}\;4\pi my\right)+4\pi my$$

$$2l_1\frac{I}{A_1} = \frac{2l_1}{A_1+A_1+2kl_1}\left(I-\frac{A_1-A_1}{2l_1}\;4\pi my\right)-4\pi ny,$$

whence

$$I-I = \frac{(A_1-A_1)I}{A_1+A_1+2kl_1}-\left\{\frac{(A_1-A_1)^2}{A_1+A_1+2kl_1}-(A_1+A_1)\right\}\frac{4\pi ny}{2l_1}$$

$$= \frac{(A_1-A_1)I}{A_1+A_1+2kl_1}+\frac{4A_1A_1+2kl_1(A_1+A_1)}{A_1+A_1+2kl_1}\;\frac{4\pi ny}{2l_1}$$

For illustration consider the simplest possible case let $b = l_1 = \frac{1}{2}A_1$ and $b_2 = l_2 = \quad = 0$ and let $2kl_1$ be negligible we have—

$$I-I = I\cos\frac{2\pi t}{T}+A_1\sin^2\frac{2\pi t}{T}\;\frac{4\pi ny}{2l_1}$$

and the equation of current will be—

$$Ry = n\left\{\frac{2\pi}{T}I\sin\frac{2\pi t}{T}-\frac{2\pi nA_1}{l_1}\frac{d}{dt}\left(\sin^2\frac{2\pi t}{T}\;y\right)\right\},$$

instead of the simple and familiar linear equation

III Transmission of Sunlight through the Earth's Atmosphere By Captain W de W Abney RE FRS

Received February 17, 1887

(Abstract)

The observations were made by means of the colour photometer which General Festing and himself introduced last year and which they described in the Bakerian Lecture for 1886 They extended over more than a year the object being to ascertain the intensity of the different rays in the solar spectrum after passing through various thicknesses of the atmosphere Owing to the unpromising results obtained by Langley with his bolometer experiments, it was not anticipated that the variation in the intensities of the different rays would obey any law, but subsequent investigation showed that as a rule the intensity of any ray obeyed the law enunciated by Lord Rayleigh in that $I = I\epsilon^{-kx\lambda^{4}}$, where I and I are the initial and transmitted wave lengths x the thickness of the medium through which the ray passed, and k a constant, λ being the wave length The

standard illuminating value of the spectrum was taken from observations made in Switzerland at 8000 feet altitude on September 15 at noon. The other observations were made at South Kensington. It was found that with the wind in the proper quarter the sky at the latter place was as pure in colour as in the country, and that measures made on the days on which there was apparently no haze gave results which when combined together gave a minimum value for k of 0.0013. A mean of the results showed that $k = 0.0017$.

The author then discusses the value of the area of the curves so obtained, and shows that astronomers who have used the ordinary logarithmic formula of $I' = Ia^{-\sec ZD}$ have not erred in so doing, but that these results are perfectly concordant with the results obtained by taking the different values of absorption over the whole visible spectrum. He further shows that with the coefficients of transmission for different wave lengths which Langley has published, the above formula is applicable, which is contrary to the theory which Langley propounded. The author further shows that when using the part of the spectrum to which various photographic salts are sensitive, the areas of the curves of intensities also fall in with the logarithmic formula.

He also points out that if the value of μ in the formula $I' = I\epsilon^{-\mu x}$ (from which the formula $I = Ia^{\sec ZD}$ is deduced) be divided by 104 in the case of the optical value or by 255 in the case of the photographic value, when bromoiodide of silver is employed on the sensitised plate, the value of k is obtained. 104 and 255 represent $1/\lambda^4$ of 5570 and 4450 respectively. From this he deduces the fact that the illuminating value of any part of the visible spectrum on any day at any hour can be ascertained by taking the optical and photographic values of total sunlight. This plan not only enables the light which undergoes general absorption to be calculated, but *also* the values of loss of intensity for each ray. He further indicates that a double photographic observation, in one of which the less refrangible end of the spectrum, and in the other of which the more refrangible is used, will give similar results.

Experiments on the photographic methods are described, and the results agree with those which theory indicated would arise.

The author states that the high altitudes in the Alps may not be suitable for observations of wave lengths in the infra-red, but that they are especially suitable for observations in the visible part of the spectrum, since the atmosphere is very often free from dust, though it may not be free from aqueous vapour, and the latter affects the dark rays vastly more than it does the visible rays. He then points out the probable cause of the presence of particles which scatter light, and briefly discusses the differences which he obtains in his value for the transmission of light, as compared with Bouguer, Seidel, Pritchard, and others He further shows that observations made by people who are colour-blind in the red tend to diminish the value of the coefficient of transmission, and that the difference between stellar and solar light cannot account for the apparent discrepancy It would appear that Professor Pritchard's maximum value for the coefficient of transmission at Cairo does not differ much from his

The values of the different colours in the spectrum which Rood has adopted from Vierordt's method are then discussed, and corrected according to the author's determination of the values on a day in June

A series of tables close the paper, in which the original observations and the deduced values are given

Presents, March 10, 1887

Transactions

Calcutta —Asiatic Society of Bengal Journal (Natural History) Vol LV Part 2 No 3 8vo *Calcutta* 1886 Journal (Philological) Vol LV Part 1 No 3 8vo *Calcutta* 1886 Proceedings 1886 Nos VIII–IX 8vo *Calcutta* The Society

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Stolipine (D) Essais de Philosophie des Sciences 8vo *Genève* 1886 The Author

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Photographs of Cape Observatory, Exterior and Interior, with Photograph of Stars about η Argus, and other Photographic Star maps Dr Gill, F R S

Mezzotinto Engraving of Thomas, Lord Bishop of Rochester, and Thomas Sprat, A M, Archdeacon of Rochester Mr Eldridge Spratt

March 17, 1887

Professor STOKES, D C L, President, in the Chair

The Presents received were laid on the table, and thanks ordered for them

The following Papers were read —

I. "A Coal-dust Explosion" By W GALLOWAY Communicated by R H SCOTT, M A, F R S Received February 17, 1887

(Abstract)

The Silkstone pits of Altoft s Colliery, near Normanton, in Yorkshire, in which the explosion took place, are 420 yards deep Both shafts are round, the down-cast being 12 feet and the up cast 10 feet in diameter The thickness of the working, including a bed of soft shale below the seam, used as a holing, is 4 feet 6 inches The system of working is longwall The number of men and boys employed underground in the day shift was about 350 The colliery is now twenty one years old

Very little fire-damp is produced in the workings Naked lights were used by all the workmen for twenty years before February, 1886, yet during the whole of that period no single workman had been injured by an explosion of fire damp, great or small

Besides being naturally very free from fire damp, the workings of this mine were continuously swept by strong and swift currents of air, produced by means of two ventilating furnaces at the bottom of the up cast shaft, amounting in the aggregate to 147,380 cubic feet per minute

As the roof subsides upon the stowing near the faces, it is necessary to take down a certain thickness of it in the stall roadways, in order to preserve them at a workable height For this purpose about 4 feet in thickness of roof was taken down by blasting in each stall road, the height being thus made about 8 feet 6 inches, at a distance of 10 or 12 feet back from the face Each stall road thus required about one blasting-shot, with a charge of from two to three pounds of powder to be fired in it about once every five or six days, so that from seven to ten blasting shots were fired near the faces every day In this way the floor of each roadway became covered with small

pieces of broken roof, which completely obscured any small quantity of coal or coal dust that might have fallen upon it, and been left there in the process of coal-getting

The tubs consist of rectangular wooden boxes mounted on wooden frames, with wheels attached to them They were filled with coal to a level with the top and then contained about 10 cwt Thus although no coal could fall over the ends or sides, the vibration due to the operation of hauling caused coal dust and small pieces of coal to be shaken out through the seams in the sides and bottom on to the roadway beneath Here it accumulated little by little between the rails, and to a distance of a few inches on each side of them, and the attrition due to the constant trampling of men and horses together with the occasional dragging of the endless chains on the floor gradually reduced it to a state of fineness The quantity of coal dust which accumulated on any roadway was thus other things being equal, proportional to the number of full tubs that had passed along it from the first, so that the oldest roadways would naturally be the dustiest were the accumulations not removed from time to time

Each return air way represents the continuation of a stall road all the way from near the bottom of the up cast shaft to the face and it must therefore contain in any given section of its length about the same average quantity of coal dust as any ordinary stall road But the return air ways are all used as travelling roads for the men and boys going to and from the faces, and the constant trampling of feet soon mixes up any little coal dust there may happen to be on the floor with the dust of the roof stuff, and reduces the whole to an impalpable powder of a light grey colour

The following conditions thus prevailed before the explosion —

1 An unusual immunity from fire damp

2 Very excellent ventilation

3 Blasting going on at the rate of say 2000 shots a year, involving the consumption of upwards of two tons of powder in the same time, and this within the zone of subsidence where practically speaking, all the fire damp was given off, and where there was no coal dust

4 Pure air filling the intake air ways from the bottom of the down cast to the faces

5 Air containing all the fire damp in the colliery filling the working places and the return air ways from the faces to the up-cast

6 Coal dust in the intake air-ways decreasing rapidly near the faces

7 Light grey dust of roof stone, but no visible coal-dust in the return air-ways

The colliery had been carried on under these conditions for seventeen years within the experience of the present manager. During the whole of that time no blasting had ever been done in any intake air-way. On the 2nd of October last, however, a party of men were instructed to blast away a portion of the side of the west chain road at a distance of about 550 yards from the bottom of the shaft. They fired three shots in all, and the third caused the explosion. It was not a blown-out shot in any sense.

The mechanical effects were exactly the same as those produced by other explosions. Timber was torn out, and falls of roof sometimes of great magnitude and extent were caused all through the intake air-ways, as far as the flame reached. In the endless chain roads the box part of each empty tub was swept away from the frame, and shattered into small pieces, not one being left whole. A spare pulley-wheel, 4 feet in diameter and weighing 15 cwt., which had been standing on its edge leaning against the side near the end of the west chain road, was carried four yards inwards towards the face, and laid flat on its side.

The flame traversed all the intake air-ways, except the new east road, and died out in some nearer to, and in others further from, the faces. It did not in any case pass into a return air-way. It did not reach the face of the workings at any point.

The new east road was quite undisturbed. Two men who were working in it felt a concussion of the air, but saw no flame, and came out unscathed. This result appears to be due entirely to the circumstance that the principal stables were ranged along the entrance to this road, and the ground having been kept constantly wet with the water used in the service of the horses, the flame was unable to pass that point for want of coal-dust to sustain it.

II. "Second Note on the Geometrical Construction of the Cell of the Honey Bee ('Roy. Soc. Proc.,' vol. 39, p. 253, and vol. 41, p. 442)." By Professor H. HENNESSY, F.R.S. Received February 21, 1887.

If from the intersections of the diagonals of the three lozenges forming the apex of the cell, perpendiculars be erected, these will meet at a point on the cell's axis, and each of them is manifestly the radius of a sphere tangent to the three lozenges. A plane passing through a radius and the axis passes through the short diagonal, e, of the lozenge whose length is $h\sqrt{(3/2)}$; using the notation and results of the paper above cited.

The distance intercepted on the axis by a perpendicular let fall from the middle of the lozenge is equal to $x = h/(2\sqrt{2})$, and as this perpendicular is manifestly equal to $\sqrt{(\frac{1}{4}e^2 - x^2)}$, and as we have evidently—

$$\frac{r}{\sqrt{(\frac{1}{4}e^2-x^2)}} = \frac{e}{2x} \qquad \text{we obtain} \qquad r = \tfrac{1}{2}h\sqrt{3}$$

But $h\sqrt{3}$ is a diameter of the hexagonal prism perpendicular to two of its opposite faces hence a sphere may be inscribed within the cell from a point measured from the vertex at a distance equal to the side of one of the lozenges and with a radius equal to half the long diagonal of this lozenge, while another sphere with a diameter equal to three times the side of the lozenge circumscribes the triangular pyramid at the summit

The diameter D of the inscribed sphere is equal to the diameter of the cell while the diameter D of the exterior sphere is equal to the sum of three edges of the pyramid, and it therefore follows from the first note vol 41 that between these diameters we have the expression—

$$\frac{\mathrm{D}}{\mathrm{D}} = \left(\frac{3}{2}\right)^{\frac{3}{4}}$$

The relation between the geometrical cell and the interior and exterior spheres whose diameters are connected by this equation may possibly have some bearing on the question of the formation of the actual cells

III The Embryology of Monotremata and Marsupialia Part I By W H Caldwell M A Fellow of Gonville and Caius College Cambridge Communicated by Prof M Foster, Sec R S Received February 22, 1887

(Abstract)*

1 *The Egg membranes*

In Monotremata in very young ova a fine membrane exists between the single row of follicular cells and the substance of the ovum This membrane which I will call *the vitelline membrane* at first increases in thickness with the growth of the ovum and through it pass numerous fine protoplasmic processes connecting the protoplasm of the follicular cells with that of the ovum, and serving to conduct food granules, which appearing in the neighbourhood of the nuclei of the cells, travel thence to the ovum; food granules also appear in the neighbourhood of the germinal vesicle, and travel away from it hence the horse-shoe shape of the yolk-mass as seen in section. The time during which food granules are thus passing from the follicular cells to the ovum may be called "the yolk forming period"

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* The author being at the present time in Australia and so unable to correct the proof of this abstract I have undertaken this duty In doing so I have ventured for the sake of what appeared to be increased clearness to introduce into § 1 some modifications of the author s manuscript being guided therein by the author s more detailed account given in the fuller paper —M Foster Sec R S

It is succeeded by a period during which the vitelline membrane again becomes thin, the follicular cells are reduced to a single layer, and the cells are very thin and flat This period may be called the "absorption of fluid period," since during it the ovum absorbs large quantities of fluid through the thin vitelline membrane and single layer of thin follicular cells, and thereby increases largely in size

This is in turn succeeded by a third period, during which the follicular cells again become active, multiply, increase greatly in size, and give rise between themselves and the vitelline membrane to a deeply staining homogeneous layer, which I will call *the chorion*. This period may hence be called "the chorion forming period' All these three periods are gone through while the ovum is still in the follicle

Upon the bursting of the follicle and the reception of the ovum in the Fallopian tube, a few of the follicular cells remain attached to the chorion, the majority are left behind within the burst follicle

During the passage along the Fallopian tube, the vitelline membrane again increases in thickness, and the chorion, also increasing in thickness, absorbs fluid and becomes *the albumen layer* Outside this now appears a new structure, *the shell* or shell-membrane, of tough parchment-like consistency,* not staining with reagents I have not yet traced the deposition of the shell to the activity of any special glands, but I can say that the shell-membrane does not increase at the expense of the chorion or albumen layer

After reaching the uterus both vitelline membrane and shell-membrane increase in thickness, but the albumen layer diminishes and disappears, serving apparently for the nutrition of the ovum Immediately beneath the vitelline membrane a new layer is now seen in hardened preparations, but it may be shown that this layer is really fluid, yielding a coagulum which stains deeply with reagents, the fluid being apparently derived, through the membranes, from the uterine glands

In Marsupialia the history of the vitelline membrane, save that "the yolk forming period" is not marked off from the "absorption of fluid" period, is similar to that in Monotremata I have not been able to trace the beginning of the "chorion" while the ovum is still in the ovary, in Marsupialia but in an ovum of Phascolarctos, from the uterus, I found a chorion like that of Monotremata, and surrounded moreover by a thin transparent membrane—a *shell membrane* Within the uterus the chorion, increasing in thickness, becomes transformed into an albumen layer, and is eventually absorbed, passing through the vitelline membrane to nourish the ovum, so that eventually the vitelline membrane comes to be close to the shell

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* In the shell of the laid egg of Echidna I have not detected calcic salts, but that of Ornithorhyncus gives rise to gas when treated with dilute acid

As in Monotremata, a coagulable, and when coagulated, deeply staining fluid makes its appearance between the vitelline membrane and ovum (blastoderm)

The shell membrane persists until the developing ovum becomes fixed to the walls of the uterus after which it disappears

The paper then compares the egg membranes just described with those of Placentalia, and those of Vertebrata generally

2 *Segmentation*

The telolecithal ova of Monotremata and Marsupialia go through a partial segmentation The ova of Placentalia segment completely, but the resulting blastodermic vesicle is identical with that produced by partial segmentation in Monotremata and Marsupialia

In Monotremata there is a posterior lip to the blastopore similar to that of Elasmobranchii The epiblast grows in so rapidly from the sides that a primitive streak region is formed in front of the posterior lip long before the epiblast has enclosed the yolk This unenclosed area in front of the primitive streak probably includes a region where the hypoblast (yolk) has secondarily broken through the epiblast The existence of such a region would hide the position of the anterior lip of the blastopore The circumference of the circle made up by the larger arc of the edge of the blastoderm on the yolk, and the smaller arc of the posterior lip of the blastopore, is a measure of the quantity of yolk in a meroblastic ovum

In Marsupialia the epiblastic growth encloses the hypoblast at a very early age, except over a narrow slit in front of the posterior lip of the blastopore This slit corresponds to the area enclosed by the circle described above in a meroblastic egg The primitive streak is not conspicuous at an early age because of the large size of the cells No hypoblast projects through the epiblast in front of the primitive streak region I would explain the segmentation and the gastrula of Placentalia in the same way Balfour s objection ('Comp Embryol,' vol 2 p 187) to Van Beneden s original comparison of the blastopore of the rabbit with that of a frog, is explained away by the presence of a posterior lip to the blastopore in Marsupialia My explanation postulates the existence of a similar structure in the rabbit The blastopore of the rabbit corresponds therefore to the whole area marked out by the growing epiblast and the posterior lip of the blastopore before the closing of the primitive streak region, or, to this area minus the secondary extension, caused by the projecting yolk, in Monotremata.

IV. "On the Total Solar Eclipse of August 29, 1886 (Preliminary Account)." By ARTHUR SCHUSTER, Ph.D., F.R.S., Professor of Applied Mathematics in Owens College, Manchester. Received March 3, 1887.

The instrument entrusted to me by the Eclipse Expedition was similar to that employed in Egypt during the eclipse of 1882. The equatoreal stand carried three cameras, one of which was intended for direct photographs of the corona, while the two others were attached to spectroscopes

Photographs of the Corona.—The lens had an aperture of 4 inches, and a focal length of 5 feet 3 inches; giving images of the moon having a diameter of about 0 6 of an inch.

During the first minute of totality the corona was covered by a cloud, which was, however, sufficiently transparent to allow the brightest parts of the corona to show on the two photographs exposed during that time

During the remaining time, that is to say, during about two minutes and a half, the sky was clear, but there were clouds in the neighbourhood of the sun.

The time of exposing the photographs, which had been fixed beforehand, had to be altered in consequence of the uncertainty of the weather, and for this reason I can only give the actual times of exposures very approximately and from memory One photograph on sensitive paper shows only little detail; but three photographs on glass were obtained, which, as regards definition, I believe to be equal to those obtained in Egypt. The approximate exposures were 15 to 20 seconds, 10 to 15 seconds, and about 5 seconds.

With the view of possibly increasing the amount of detail, which it has hitherto been possible to obtain on the photographs of the corona, I have, on this occasion, given considerable attention to the different adjustments, so as to fix the cause which at present limits the definition. A telescope of 4 inches aperture ought to separate two small objects which are at an angular distance of about 6 seconds of arc. The theoretical resolving power can only be realised with small and distinct objects like double stars; in an object like the corona it is difficult to estimate the resolving power actually obtained. Nevertheless, as far as I can judge, the photographs obtained in Egypt and in Grenada with the same instrument show no detail which theoretically could not have been seen with an aperture of 1 inch. The photographs obtained in India during the eclipse of 1871 show about the same fineness of detail as those obtained in 1882 and 1886, but the aperture then employed was only 2 inches It appears from this either that the corona does not as a rule show sufficient detail to warrant an aperture greater than 2 inches, or that for some other reason the full advantage of an aperture of 4 inches has not been realised

A linear object having an angular magnitude of 6 seconds of arc in a camera of 63 inches focal length would have a length of about the 600th part of an inch in the photographic plate Remembering the fact that diffraction gratings have been photographed with 17,000 lines to the inch, it is clear that it cannot be the fault of the photographic film if greater definition is not obtained

The different adjustments which it is in th power of the observer to regulate can all be easily made to the required accuracy I feel certain, at any rate that during the last eclipse the orientation of the instrument and the average rate at which the clock went during the eclipse were sufficient to give the theoretical resolving power There seems to me to be only one way in which an improvement in the photographs can be hoped for The mechanical arrangement by means of which the clock motion is transferred to the camera is far from perfect The series of cogged wheels which transmit the motion must do so in my opinion, in an irregular and oscillatory manner, so that although the average rate of the clock can be regulated with sufficient accuracy, there are minor periods which seriously interfere with the regularity of the motion and the definition of the images If we are to obtain better photographs of the corona we can only hope to do so by means of a better mechanical arrangement for moving the camera

Photographs of the Spectrum of the corona were obtained by means of two instruments one being identical with that employed at Caroline Island in 1883 This spectroscope has two prisms of 62° refracting angle, the theoretical resolving power being about 10 in the yellow (The unit of resolving power is that resolving power which allows of the separation of two lines differing by the thousandth part of their own wave length) The slit of this spectroscope was placed so that it was tangential to the image of the sun formed by the condensing lens One plate was exposed during the whole of totality The results are good, a number of lines belonging to the prominences and to the corona are very distinct and can be measured with fair accuracy The difficulty of measurement lies in the multitude of lines I have measured at present between forty and fifty distinct corona lines between the solar lines F and H, and a number remain unmeasured

A comparison between the photographs of 1882 and 1886 shows that although the lines seem to be in the same position their relative intensity has greatly altered The strongest corona line during the last eclipse had a wave length of about 4232 it is slightly but distinctly less refrangible than the strong calcium line at 4226

The measurement of the photographs is very fatiguing to the eyes and it is only when these are in exceptionally good condition that the work can be done with any degree of accuracy The delay in bringing out a full report is solely due to this difficulty

The second spectroscope had its slit placed so as to take a radial section of the corona It had one large prism giving a theoretical resolving power of 11 4 slightly larger therefore than the two prism spectroscope

The film was one prepared by Captain Abney so as to be more sensitive in the green than the ordinary plates

The photograph obtained is faint but I believe will ultimately give good results

A good drawing of the corona was obtained by Captain Maling at the station occupied by Captain Darwin and myself

The plates were prepared by Captain Abney whose valuable help I have had in the whole of the preliminary arrangements

Presents March 17 1887

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Jones (T Rupert), F R S History of the Sarsens 8vo [*Devizes*] 1887 The Author

Scacchi (A) I Composti Fluorici dei Vulcani del Lazio 4to [*Napoli*] 1887, Le Eruzioni Polverose e Filamentose dei Vulcani 4to *Napoli* 1886, Sopra un Frammento di Antica Roccia Vulcanica 4to *Napoli* 1886 The Author

Tavitian (S) De l E ou du Positif de l Être qui est l Objet de la Science Positive 8vo *Paris* 1887 The Author

"Contributions to the Chemistry of Chlorophyll No. II." By EDWARD SCHUNCK, F.R.S. Received November 25,—Read December 16, 1886.

[PLATE 1.]

Considering the intimate though as yet little understood connexion between chlorophyll and the carbon dioxide of the atmosphere, it seemed to me that it might be of interest to ascertain whether compounds of phyllocyanin, similar to those previously described, could be obtained in which the organic or other acid should be replaced by carbonic acid.

On passing a current of carbon dioxide through a solution of phyllocyanin in absolute alcohol holding a quantity of freshly precipitated hydrated ferrous oxide in suspension, no combination took place, the phyllocyanin remaining unchanged, though the current of gas was passed through the liquid for a considerable time.

On substituting freshly precipitated cupric oxide for the ferrous oxide there were slight indications of some reaction taking place, but the phyllocyanin remained for the most part uncombined.

A much more decided effect is produced when zinc oxide is employed. On passing a current of carbon dioxide for several hours through an alcoholic solution of phyllocyanin holding hydrated zinc oxide in suspension, the colour of the liquid changes by degrees, the dull phyllocyanin tint disappearing. On being filtered from the excess of zinc oxide (which retains a pale green tint not removable by washing with alcohol) it appears of a fine bluish green; it fluoresces strongly, and shows absorption bands which coincide with those of the alcoholic solution of phyllocyanin zinc acetate. The liquid leaves on evaporation a dark green semi-crystalline residue, which may be heated to 150° without undergoing any change, but is decomposed at a temperature of 160—170°. It is, however, very easily decomposed by the action of strong acids. On adding a little hydrochloric acid to its alcoholic solution and heating, bubbles of gas are seen to escape, the colour of the solution changes to blue, and on now adding water there is a dull flocculent precipitate, which dissolves easily on shaking up with ether, the solution showing the colour and absorption bands peculiar to phyllocyanin solutions. There can be no doubt, therefore, that the compound formed is a phyllocyanin zinc carbonate, which by the action of strong acids is decomposed, yielding phyllocyanin and carbon dioxide. An alcoholic solution of the compound exposed in an open test-tube to sunlight soon loses its bright-green colour and becomes yellow, the absorption-bands at the same time disappearing.

The compounds of phyllocyanin containing zinc are in some respects not unlike chlorophyll itself, or rather the green constituent of chlorophyll, and it was, therefore, natural to suppose that the peculiar effects observed when zinc powder was made to act on Hoppe Seyler's chlorophyllan (impure phyllocyanin?) were due to regenerated chlorophyll. The phyllocyanin zinc compounds, like chlorophyll itself, yield bright green strongly fluorescent solutions; they are easily decomposed by strong acids and they are rapidly changed under the combined influence of air and light. The absorption spectrum of the phyllocyanin zinc compounds consists like that of chlorophyll of four bands, but the bands do not exactly coincide with those of chlorophyll as regards position and relative intensity. In making such comparisons it must not of course be forgotten that what we in a general way call chlorophyll is a mixture the components of which have not hitherto been separated from one another in a satisfactory manner.

The various double compounds of phyllocyanin are not equally susceptible to the influence of air and light; they differ as much in this respect, *inter se*, as do phyllocyanin and chlorophyll, the former being a very stable body, the latter exceedingly unstable. The compounds containing zinc are most easily changed, while those containing copper are very permanent. These differences were clearly manifested in an experiment of which the results were as follows:—A dilute alcoholic solution of phyllocyanin was divided into four equal parts. In one part the phyllocyanin was left uncombined; in the second part it was by the addition of acetic acid and zinc oxide and boiling changed into phyllocyanin zinc acetate; while in the third and fourth parts it was in like manner converted into phyllocyanin ferrous acetate and phyllocyanin cupric acetate respectively. All four liquids having been made equal in volume by the addition of alcohol where required, were exposed from the end of July in loosely stoppered cylinders to alternate sunlight and diffused daylight, an alcoholic solution of chlorophyll of approximately the same intensity as the phyllocyanin zinc acetate solution being placed in a fifth vessel at the side. After three days' exposure the chlorophyll solution had become nearly colourless though it still showed bands due to ready-formed phyllocyanin. After one week's exposure the phyllocyanin zinc acetate solution had in like manner lost the greater part of its green colour, retaining only a faint greenish-yellow tinge with a slight fluorescence, and showing a faint band in the red. After a fortnight's exposure the solution appeared nearly colourless, and the band in the red could only be seen when the light passed through a considerable thickness of liquid, while the other solutions were quite unaltered as regards colour and absorption bands. After a further exposure of five weeks the phyllocyanin solution had become much paler, though still showing the characteristic absorption bands, while in the solution of the ferrous compound only a faint indication of the band in the red could be seen, the solution of the cupric compound remaining quite unchanged as regards intensity of colour and distinctness of absorption. After a still further exposure the absorption-bands of the phyllocyanin solution had nearly disappeared, only a faint indication of the one in the red remaining, but no change could be perceived in the solution of the cupric compound. It appears, therefore, that while the zinc compound shows far less stability when exposed in solution to the action of air and light than uncombined phyllocyanin, being indeed almost as unstable as chlorophyll itself, the cupric compound is far more stable, the ferrous compound occupying in this respect a position intermediate between that of the zinc compound and that of uncombined phyllocyanin. There are few compounds of colouring matters which would withstand the combined action of light and oxygen for so long a period as phyllocyanin cupric acetate. The instability of the zinc compound must be due to the nature of the compound itself, for it is difficult to suppose that the metallic base can in any way assist in the process of oxidation, for such the process must be.

Action of Caustic Alkali and Zinc on Phyllocyanin.—When phyllocyanin is treated with boiling caustic potash lye to which zinc powder is added, it dissolves rapidly, yielding a dark greenish blue solution, which after filtration from the excess of zinc, gives with acetic acid a green precipitate. This precipitate, after filtration and washing, dissolves in ether, giving a fine bluish green fluorescent solution, which resembles the solutions of phyllocyanin zinc acetate, but shows a slightly different spectrum, all the bands except the first being further away from the red. The ethereal solution leaves on spontaneous evaporation an amorphous residue, which is green by transmitted, purple by reflected light. When instead of acetic acid hydrochloric acid is added to the greenish blue alkaline solution, there is a dark green precipitate, which differs from phyllocyanin inasmuch as its ethereal solution is greener and far more fluorescent than an ethereal solution of the latter, and shows a spectrum of five bands, all of which lie further away from the red than the corresponding ones of phyllocyanin. It is probable that the precipitate with hydrochloric acid consists of a product formed by the reducing action of zinc on phyllocyanin, whereas the precipitate with acetic acid represents the double compound of this body with zinc and acetic acid, being formed when acetic acid is added to the solution containing the product of reduction along with zinc oxide.

Action of Hydrochloric Acid and Metallic Tin on Phyllocyanin.—When metallic tin is added to a solution of phyllocyanin in concentrated hydrochloric acid, the solution on standing gradually loses its bright bluish-green colour, which changes to a dull olive-green The addition of water to the solution produces a brown precipitate, which being filtered off and washed to remove the acid, dissolves much more easily in boiling alcohol than phyllocyanin, giving a dark olive coloured solution This solution leaves on evaporation a brown amorphous residue, which treated with ether dissolves for the most part The ethereal solution has a green colour with a tinge of red, and shows a spectrum consisting of eight bands, it leaves on evaporation a semi crystalline residue, which is green by transmitted purple by reflected light On the addition of concentrated hydrochloric acid it separates into two distinct layers, the upper one being colourless, while the lower one is green and shows four bands

By the prolonged action of tin on the solution of phyllocyanin in hydrochloric acid, a further and more complete change is effected the solution acquiring a bright red colour without the least tinge of green On adding water the solution gives a red flocculent precipitate, which filtered off and washed appears dark red the filtrate retaining an orange colour, and showing a broad ill defined band, covering part of the green and blue The precipitate dissolves almost entirely in warm alcohol, giving a bright red solution This solution, on adding a little caustic alkali, acquires a bright lemon yellow colour, but the red colour is restored by an excess of hydrochloric acid the alkaline solution shows a broad band between the green and blue but on standing a short time exposed to the air it changes becoming of a deeper yellow, and it now shows a spectrum of three bands one in the yellow, one in the green, both pale, with a dark band between the green and blue The red alcoholic solution leaves on spontaneous evaporation a reddish brown amorphous residue, which dissolves only in part in boiling ether, giving a reddish brown solution, which shows a peculiar spectrum of six bands The body showing this spectrum is probably a product of oxidation of the red substance The acid liquid filtered from the red precipitate, after removing the tin with sulphuretted hydrogen, is found to contain a mere trace of organic matter

The colouring matter formed by the action of tin and hydrochloric acid on phyllocyanin resembles in some respects those contained in the petals of red flowers Hitherto, however, I have not succeeded in finding among the latter any one exactly like it

Examination of the Mother liquors of Phyllocyanin —Having some reason to suspect that the acetic acid mother liquors of phyllocyanin contained some other substance similar to it of definite character, I added water to a portion of these liquors, so as to throw down the whole of the colouring matter contained in it The flocculent precipitate was filtered off and dissolved in boiling alcohol The solution on cooling gave a crystalline deposit which was found to possess all the properties of phyllocyanin. I think therefore that the mother-liquors contained that colouring matter only, along with fatty matters and other impurities.

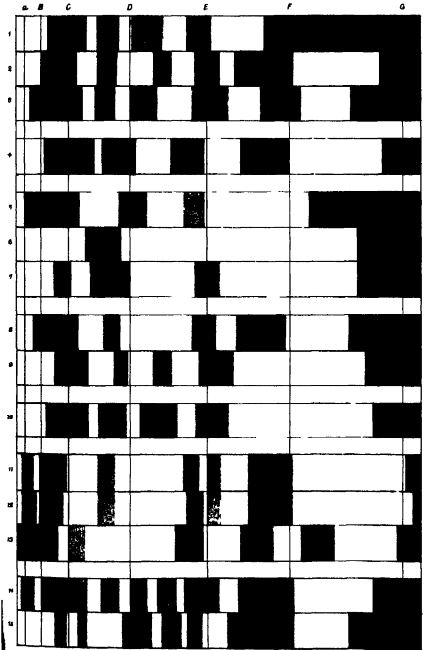
EXPLANATION OF PLATE.

Absorption Spectra of Phyllocyanin and its principal Compounds and Derivatives.

1. Chlorophyll in alcohol. This spectrum is given merely for the sake of comparison, not as a new representation of what has so frequently been depicted by others
2. Phyllocyanin in ether.
3. Phyllocyanin in concentrated hydrochloric acid.
4. Phyllocyanin cupric acetate in ether.
5. Phyllocyanin ferrous palmitate in alcohol.
6. Phyllocyanin ferrous palmitate after treatment with hydrochloric acid in the cold
7. Phyllocyanin ferrous palmitate after treatment with boiling hydrochloric acid.
8. Phyllocyanin ferrous malate in alcohol
9. Phyllocyanin ferrous malate after treatment with hydrochloric acid.
10. Phyllocyanin zinc carbonate in alcohol. This spectrum should be compared with that of chlorophyll
11. Product obtained by treating phyllocyanin with caustic alkali, then with acetic acid, in ether.
12. Product formed by the further action of acetic acid on the preceding, in ether.
13. Product of the action of alcoholic potash or soda on phyllocyanin, in ether.
14. First product of the action of tin and hydrochloric acid on phyllocyanin, in ether.
15. Final product of the action of tin and hydrochloric acid on phyllocyanin, in ether.

Schunck

Proc Roy Soc Vol 42 Plate I



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March 24, 1887.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Major A. W. Baird, R.E. (elected June 4, 1885), was admitted into the Society.

The following Papers were read:—

I. "Preliminary Note on the 'Radio-micrometer,' a New Instrument for Measuring the Most Feeble Radiation." By C. V. BOYS, Demonstrator of Physics at the Science Schools, South Kensington. Communicated by Professor A. W. RÜCKER, F.R.S. Received February 24, 1887.

Till lately the thermopile and galvanometer have afforded the most delicate means of detecting and measuring radiant energy. This combination has been surpassed by Professor Langley, who has made use of the increased resistance of metallic wires or the diminished resistance of a carbon filament when warmed.

It seems difficult to believe, in consequence of the very small change of resistance which even iron undergoes when slightly warmed, that this is the best principle to make use of in designing the most sensitive possible instrument. I felt that if an instrument depending on thermo-electric force could be made as beautifully as Professor Langley's bolometer, a better result ought to be obtained. The one point in which the bolometer has a great advantage is the small mass of the part to be heated, whereas a thermopile, delicate as the bars may be, has a mass so enormous that both the rate of heating and the ultimate rise of temperature must be small in comparison.

A thermopile with bars as thin as the wires of the bolometer cannot be made with antimony and bismuth, and yet such a construction would be required before the thermo-electric force could be utilised to the same extent that the change of resistance is in the bolometer.

If the conducting wires had no resistance, no advantage would be gained by having more than one junction, provided that the galvanometer were properly proportioned. If it were not for the resistance and torsion of the stretched wires, the moving coil galvanometer of

Deprez (on the principle of Sir W Thomson's siphon recorder) would have many advantages It occurred to me that if an active bar were made with two pieces of metal, antimony and bismuth, as thin as possible, soldered edge to edge, and if the outer edges were joined by an arch of copper, and if this were hung in a strong magnetic field, all the advantages of an ideal thermopile and of the Deprez galvanometer would be gained, while the impossibility of the former, and the resistance and tension of the stretched wires of the latter, and the resistance of the wires connecting the thermopile with the galvanometer would be abolished I anticipated that a greater delicacy would be obtained than has been considered possible, that the instrument would be convenient in that it would be of necessity dead beat, that its indications would be proportional to the received radiations, that it would have the constant and definite zero which mechanical control such as torsion gives, that it would not be affected by the magnetism of objects near it, and that as the circuit could be suspended in a hole in a mass of metal with one window for the radiation to enter, the instrument should be very insensitive to temperature changes other than those in the line of action It has the further advantage that for spectroscopic work the radiation may be limited by a narrow slit without much reducing its sensibility * It is, however, a fixed instrument that cannot be handled and pointed like the thermopile or the bolometer

I have made some preliminary experiments to test these conclusions One instrument, made by my friend Mr Cunynghame very roughly with a torsion wire support only a few inches long, is fairly sensitive

One which I made with the active bar about 6×8×⅛ *mm*, and with a circuit 1 cm square, of one turn of which the three sides are made of copper wire about ¼ mm in diameter, and in which the motion of the circuit is resisted by a torsion fibre of the finest spun glass about 10 cm long, gives when placed on the poles of a permanent magnet results so good that I have ventured to submit the instrument for inspection to the Royal Society This particular instrument is capable of showing the heat which would be cast by a candle flame on a halfpenny at a distance of 200 yards † As I have not yet employed what I know to be the best proportions, it is evident that very great sensibility is possible

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* I should have mentioned that much larger angles could be measured if the fibre were suspended by a torsion head and the light brought to a definite position for every observation —[*March* 2 1887]

† This same instrument with a 38 cm fibre in a field of about 100 units only, gave a deflection of 4 5 cm when the heat cast by a candle flame 12 feet off fell on the active bar which was exposed over a surface of 4 × 6 mm, therefore a candle 254½ feet distant would produce a visible effect, or the heat which would fall on a halfpenny 1168 feet from a candle flame would be sufficient to be observable if it were directed on to the active bar —[*March* 2, 1887]

It is easy to calculate exactly what deflection will be caused by a given rise of temperature in any instrument. Taking quantities, all of which can be easily obtained, namely, antimony and bismuth, each 5×5×0·25 mm., one arch of copper wire one eighth of a square mm. in section, completing a circuit of one square cm., suspended in a field of 10,000 units by a thread of such a torsion as would give an undamped period of oscillation of 20 seconds, the least movement that could be detected would be produced by a rise of temperature of about one ninety-four millionth of a degree centigrade. It seems to be capable of attaining about 100 times the sensitiveness of the bolometer. Further the electromotive force acting at this temperature would be only about one million millionth of a volt, which is probably smaller than any that can be detected by other means.

I am now engaged in working out such details as the various maxima. Thus for a certain shape of circuit and number of turns a certain thickness of copper gives the best result, of various shapes the rectangle only is practically convenient, but taking the best thickness of wire for each successive length, a certain length is better than any other, with a particular shape and the best thickness two turns are better than one or three but it does not follow that this will be so for all shapes no advantage is gained by employing more than one junction. An increased sensibility may be produced by either using a longer torsion thread or a stronger field a certain relation between these and the resistance will give a maximum quickness.

The conductivity of aluminium compared to its mass is as is well known, more than that of any other material, so it would be preferable to copper if it were not for the difficulty of soldering it.

The instrument may be made with a second active bar, forming the other end of the rectangle, with its own window, and can thus be used differentially, one bar being exposed to the radiation to be measured and the other to any compensator.

It is interesting to note that the damping action is slightly reduced by the Peltier effect.

I anticipate some difficulty in using the field produced by an electromagnet, for even if the effect of the heating of the coils by the current can be avoided as I think it may by the use of long iron connecting rods passing through water, any variation of current will give rise to motions of the suspended circuit, the nature of which I described in the 'Phil. Mag., September, 1884. But after these difficulties are removed, there will remain the diamagnetism of the three metals and the great damping action. With fields of reasonable strength, the diamagnetism may be set against the torsion of the thread, so that but feeble controlling force may be obtained by a thread of moderate length, but for electromagnetic fields I propose to paint the circuit with a solution of a salt of iron until it is magnetically neutral

Mr Cunynghame suggested to me when I explained to him my views, that a rotating instrument might possibly be made like Crookes radiometer After some trouble I have devised and made a most simple rotating pile It consists of a cross with bismuth arms and an antimony centre To the end of each arm is soldered a piece of copper wire, the four wires being parallel to one another, and at right angles to the plane of the cross The four free ends of the wires are soldered to a ring of copper wire parallel to the cross If this is balanced on a point between the poles of a permanent magnet, and if radiant heat is allowed to fall on the right hand side of the cross, looking from the north to the south pole, the cross will at once begin to oscillate, making larger and larger oscillations until it rotates, which it will do indifferently in either direction If the heat falls upon the left hand side any motion that it may have is at once arrested It follows that if the source of heat is removed and the cross is turned mechanically, the right hand side will be cooled and the left warmed

If the cross is made with antimony arms and a bismuth centre, then what was true of the right hand side is now true of the left, and *vice versâ*

Instead of a cross and four wires I think an antimony disk, with many pieces of bismuth and many wires or with a ring of bismuth and many wires forming a sort of drum armature would give a better result, but the one described with four arms rotates rapidly when the spark at the end of a blown out match is held near it

We have in this rotating pile, which might be called an electric radiometer, an electromagnetic motor which is almost an exception to the axiom that such an engine cannot be made without sliding or liquid contacts

I am now working out fully the conditions of maximum sensibility, and hope to communicate shortly a paper to the Royal Society, in which they are discussed, and at the same time to show an instrument of the best construction and some results obtained with it

Note added March 23, 1887

I have found, both by calculation and experiment, that the movement of the circuit becomes perfectly dead beat with a field of a little over 1000 units, therefore advantage is gained by using more than one junction, as a stronger field can then be used before the motion becomes dead beat

I have spoken of a fine glass fibre as a torsion support Since this was written I have discovered a method of making fibres with a perfect elasticity, and so have avoided the inconvenience of the shifting zero of the glass fibre, and with a torsion, if required, ten million times less than that of spun glass This will be described at the next meeting of the Physical Society

II ' Note to a Memoir on the Theory of Mathematical Form ('Phil Trans' 1886 (vol 177) p 1) ' By A B Kempe, M A, F R S

Received February 26, 1887

An interesting letter of criticism from Professor C S Peirce on my recently published Memoir on the Theory of Mathematical Form has led me to reconsider certain paragraphs therein relating to the definition of what I have termed "aspects," and I am anxious to make the following amendments

For Section 5 substitute—

5 In like manner some *pairs* of units are distinguished from each other, while others are not Pairs may in some cases be distinguished even though the units composing them are not Thus the angular points of a square are undistinguishable from each other and a pair of such points lying at the extremities of a side are undistinguished from the three other like pairs but are distinguished from each of the two pairs arrived at by taking the angular points at the extremities of the diagonals, which pairs again are undistinguishable from each other Further, though two units a and b are undistinguishable from each other, an absence of symmetry may cause ab to be distinguished from ba Thus if we put aside differences arising from their positions on the paper and the use of reference letters (Secs 41 and 42) the three black spots, a, b c, shown in fig 1, are undistinguished from each other but ab is distinguished from ba, for when we take the spots a, b, in the order ab an arrow proceeds *from* the first spot *to* the second but when we take them in the order ba an arrow proceeds *to* the first spot *from* the second

For Section 7 substitute—

7 Again, there are distinguished and undistinguished triads, tetrads, m ads, . n ads, , every m ad being of course distinguished from every n ad Just as we may have ab distinguished from ba, though a is undistinguished from b, so we may have $pqrst$ uv distinguished from $qusvt$ rp, though the units p, q r, s, t, u, v are all undistinguished from each other, and further, though their pairs are also undistinguished, as likewise their triads, &c Here $pqrst$. uv and $qusvt$ rp will be termed, as in the case of pairs, different *aspects* of the collection p, q, r, s, t, u, v An aspect will be fully defined and con-

sidered in Secs 73*ff*, and we shall there see that an aspect of a collection of n units, *i e*, of an n ad, is itself an n ad. It will be convenient to speak of the aspects of the pairs, triads &c contained in a collection, as if they were themselves pairs, triads, &c of the collection.

For Sections 73 to 77 substitute—

78 If with each unit of a collection of n units a b c of any form, we mentally associate one of a given discrete heap (Sec 37) of n marks so that each unit is thereby distinguished from each of the others and from all other units, we obtain what I have, in Secs 6 and 7 termed an *aspect* of a b c

It may assist in arriving at a proper idea of the use and importance of aspects, and may justify the use of the expression "aspect," to point out that when, in the consideration of a collection of units we give different degrees of prominence in our minds to different units of the collection, we are mentally affixing distinctive marks to those units and have a particular "aspect" of the collection before our mind's eye. It may be, in some cases, that we give a distinctive degree of prominence to one unit only of the collection, and equal degrees of prominence to the rest: thus, when we consider the relations of a number of things to a particular thing, we affix a distinctive mental mark to the latter thing, and marks undistinguished from each other to the former things. It is convenient, however, for our present purposes to confine the use of the word "aspect" to the case in which a different degree of prominence, or other distinctive mental mark, is affixed to each unit of the collection.

The n marks may be associated with the n units, a, b, c in $\underline{|n}$ different ways, giving rise to $\underline{|n}$ aspects of a b, c, , each aspect being derivable from the others by transpositions of the marks. When we regard a unit a as associated with a particular mark g in a particular aspect of a, b, c, , we deal with a unit A, which is a different unit from a, and may be called an aspect of a. Thus an aspect of the n-ad, a, b, c, . , is an n ad composed of n unit aspects A, B, C,

The unit aspects composing one aspect of a, b, c, are each distinct units from those composing another aspect, and thus the $\underline{|n}$ aspects of a b c, derived by employing a particular collection of n marks, furnish in all $n\underline{|n}$ unit aspects such as A.

Since the marks employed are all distinguished from each other, the n unit aspects A, B, C, composing a particular aspect of a, b c, are all distinguished from each other.

We may associate the same collection of n marks with any number of collections of n units a, b, c, , p, q, r, , &c, and obtain $\underline{|n}$ aspects of each collection.

In future when we compare aspects of a collection, or of a number of collections of the same number of units, unless the contrary is manifestly the case, it will be supposed that the same collection of n marks is employed in each case

We may choose as our discrete heap of n marks a collection of n 'relative positions or *sorts of places*, which are distinguished from each other and from all other relative positions e g , we may choose the sorts of places—first second third, &c in a row In such a case the units $a, b, c,$ may be regarded as *occupying* the sorts of places which serve as marks

74 If two collections of units $a, b, c,$ and $p, q, r,$ are undistinguished from each other they may be regarded as corresponding to each other in one or more ways, in each of which correspondences to each unit, pair, triad, &c , of one collection there corresponds in the other a counterpart unit, pair, triad, &c , undistinguished from the former in any circumstance In any one of those correspondences two corresponding units may be regarded as occupying corresponding places, or, as we may express it, places of the same sort and we may if we please regard these sorts of places as distinguished from each other and from all other sorts of places i e , we may regard the correspondence as giving rise to two aspects, A, B, C, and P, Q R, , the former an aspect of $a, b, c,$ the latter of p $q, r,$ The two aspects A B C, and P, Q R, are clearly undistinguished from each other and, as the units of each are distinguished from each other, to each unit of one there corresponds one unit, and one only, in the other which is undistinguished from it

75 Two aspects will not be undistinguished unless they can be regarded as derived in the manner indicated in the last section, and therefore the undistinguishableness of two aspects A, B, C, and P, Q, R, indicates the existence of a definite correspondence between the two undistinguished collections $a, b, c,$ and $p, q, r,$ in which correspondence to each unit pair, triad, &c , of the one there corresponds in the other a counterpart unit, pair, triad, &c , undistinguished from the former in any circumstance

Similarly, the undistinguishableness of two aspects of the same collection a $b, c,$ may be said to indicate a *self correspondence* of the collection, i e , a correspondence in which to each unit, pair, triad, &c , of the collection $a, b, c,$ there corresponds the same, or another unit, pair, triad, &c , of $a, b, c,$ undistinguished from the former in any circumstance

76 In future, when a *correspondence* of two collections, or a *self-correspondence* of a collection, are spoken of, correspondences such as those described in the preceding sections are intended to be referred to, unless the contrary is expressly stated, as in Sections 162 to 169

77. We may regard an aspect A, B, C, ... in the aggregate as a single unit V, which may be termed a *unified aspect* of *a*, *b*, *c*, ...

Section 167 is erroneous, and should be omitted.

I may add the following errata:—

In Sec. 69, line 3, for "undistinguished" read "distinguished."
" " 122, " 1, " "aspects" " "collections."
" page 43, footnote, " "Grassman" " "Grassmann."
" " 56 " " "Pierce" " "Peirce."
" Sec. 385, line 8, " "m units" " "r units."

III. "On Ellipsoidal Current Sheets." By HORACE LAMB, M.A., F.R.S., Professor of Pure Mathematics in the Owens College, Victoria University, Manchester. Received March 2, 1887.

(Abstract.)

This paper treats of the induction of electric currents in an ellipsoidal sheet of conducting matter whose conductivity per unit area varies as the perpendicular from the centre on the tangent plane, or (say) in a thin shell of uniform material bounded by similar and coaxial ellipsoids. The method followed is to determine in the first instance the normal types of free currents. In any normal type the currents decay according to the law $e^{-t/\tau}$; the time-constant τ may be conveniently called the "modulus of decay," or the "persistency" of the type.

When the normal types and their persistencies have been found, it is an easy matter to find the currents induced by given varying electromotive forces, assuming these to be resolved by Fourier's theorem, as regards the time, into a series of simple harmonic terms. Supposing then that we have an external magnetic system whose potential varies as e^{ipt}, we can determine a fictitious distribution of current over the shell, which shall produce the same field in the interior. If $\bar{\phi}$ denote the current-function for that part of this distribution which is of any specified normal type, ϕ that of the induced currents of this type, it is shown that

$$\phi = -\frac{ip\tau}{1+ip\tau}\bar{\phi},$$

where τ is the corresponding persistency of free currents. When $p\tau$ is very great this becomes

$$\phi = -\bar{\phi},$$

in accordance with a well-known principle.

This method can be applied to find the currents induced by rota-

tion of the shell in a constant field it being known from Maxwell's 'Electricity § 600, that the induced currents are the same if we suppose the conductor to be fixed and the field to rotate in the opposite direction When the conductor is symmetrical about the axis of rotation the current function of any normal type contains as a factor cos $s\omega$ or sin $s\omega$ where ω is the azimuth and s is integral (or zero) When we apply Maxwell's artifice the corresponding time factor is $e^{\iota pt}$ where p is the angular velocity of the rotation, and we easily find that the system of induced currents of any normal type is fixed in space, but is displaced relatively to the field though an angle

$$\frac{1}{s}\ \text{arc tan}\ p s \tau$$

in azimuth in the direction of the rotation

In the most important normal types the distribution of current over the ellipsoid is one which has been indicated by Maxwell (Electricity § 675) as giving a uniform magnetic field throughout the interior For instance the axes of coordinates being along the principal axes a b c we may have

$$\phi = \mathrm{C}z, \tag{1}$$

and the corresponding persistency is

$$\tau = (4\pi - \mathrm{N})\frac{\epsilon}{\rho}\frac{a^2b^2}{a^2+b^2}, \tag{2}$$

where $$\mathrm{N} = 2\pi abc \int_0 \frac{d\lambda}{(a^2+\lambda)^{\frac{1}{2}}(b^2+\lambda)^{\frac{1}{2}}(c^2+\lambda)^{\frac{3}{2}}} \tag{3}$$

ρ denoting the specific resistance of the material and ϵ the small constant ratio of the thickness of the shell to the perpendicular on the tangent plane There is a difference of electric potential over the shell, viz we have

$$\tag{4}$$

T -2 M12

where

L, M being obtained from (3) by interchanging a and c or b and c, respectively This implies a certain distribution of electricity over the outer surface of the shell

Some special forms of the ellipsoid (*e g*, a sphere, or an elliptic cylinder) are considered and the formula (2) shown to agree with the results obtainable in these cases in other ways

The problem of induced currents due to simple harmonic variation of a uniform field, or to rotation of the shell in a uniform and constant field, is then solved; and the results are found to agree with the general theory above sketched.

In the higher normal types the current-function ϕ is a Lamé's function, degenerating into a spherical harmonic when two of the axes of the ellipsoidal shell are equal. This case alone is further discussed in the present paper; the persistency of each normal type is found, and various particular cases are considered. Of the special forms which the conductor may assume, the most interesting is that in which the third axis (that of symmetry) is infinitesimal, so that we have practically a circular *disk*, whose resistance ρ' per unit area varies according to the law

$$\rho' = \rho_0' \surd\{1-r^2/a^2\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

where ρ_0' is the resistance at the centre, a is the radius, and r denotes the distance of any point from the centre. In any normal type of free currents the current-function is of the form

$$\phi = \mathrm{C} \, . \, (1-\mu^2)^{\frac{1}{2}s} \frac{d^s \mathrm{P}_n(\mu)}{d\mu^s} \left.\begin{matrix}\cos \\ \sin\end{matrix}\right\} s\omega, \quad . \quad . \quad . \quad . \quad (6.)$$

where $$\mu = \surd\{1-r^2/a^2\},$$

provided $n-s$ be *odd*; in other words, the current lines are the orthogonal projections on the plane of the disk of the contour-lines of a zonal ($s = 0$) or tessaral harmonic, drawn on the surface of a concentric sphere of radius a. The corresponding persistency is

$$\tau = \frac{\pi^2 a}{\{n(n+1)-s^2\}\rho_0'} \cdot \frac{|\underline{n-s}}{|\underline{n+s}} \cdot \left\{\frac{1 \, . \, 3 \, . \, . \, . \, (n+s)}{2 \, . \, 4 \, . \, . \, . \, (n-s-1)}\right\}^2 . \quad (7.)$$

In the most persistent type of free currents we have $n = 1$, $s = 0$, and therefore

$$\tau = \frac{\pi^2 a}{2\rho_0'}.$$

This result is of some interest, as showing that the electrical time-constant for a disk of *uniform* resistance ρ_0' must at all events be considerably less than $4{\cdot}93 \, a/\rho_0'$.*

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* I find by methods similar to those employed by Lord Rayleigh for the approximate determination of various acoustical constants, that the true value lies between $\pi a/\rho'$ and $2{\cdot}26 \, a/\rho'$. For a disk of copper ($\rho = 1600$ C.G.S.), whose radius is a decimetre and thickness a millimetre, the lower limit gives 0·0014 sec. For disks of other dimensions the result will vary as the radius and the thickness conjointly. I hope shortly to publish the details of the investigation on which those estimates are founded.

The problem of induced currents is then discussed, and I consider more particularly the case of a circular disk, of the kind indicated, rotating in any constant magnetic field In view of the physical interest attaching to the question, it would be interesting to have a solution for the case of a *uniform* disk, but in the absence of this, the solution for the more special kind of disk here considered may not be uninstructive

As in all our calculations relating to ellipsoids of revolution, we employ elliptic coordinates, viz, seeking the origin at the centre of the disk, and the axis of z perpendicular to its plane, we write

$$\left.\begin{aligned} x &= a\sqrt{(1-\mu^2)}\sqrt{(\zeta^2+1)}\cos\omega \\ y &= a\sqrt{(1-\mu^2)}\sqrt{(\zeta^2+1)}\sin\omega \\ z &= a\mu\zeta \end{aligned}\right\} \qquad (8)$$

where μ may range from 1 to 0, and ζ from zero (its value at the disk) to ∞ The magnetic potential Ω due to the field may be supposed expanded, for the space near the disk, in a series of terms of the form

$$\bar{\Omega} = \bar{\mathrm{A}}\,(1-\mu^2)^{\frac{1}{2}s}\,(\zeta^2+1)^{\frac{1}{2}s}\,\frac{d^s\mathrm{P}_n(\mu)}{d\mu^s}\,\frac{d^sp_n(\zeta)}{d\zeta^s}\left.\begin{matrix}\cos\\ \sin\end{matrix}\right\} s\omega, \qquad (9)$$

where P_n is the zonal harmonic, and p_n a similar function in which all the terms are +, instead of alternately + and − *

The terms for which $s = 0$ are symmetrical about the axis, and produce no currents, but only a certain superficial electrification The density of this is calculated for the particular case $n = 1$, i.e., for the case of a disk rotating in a *uniform* field about an axis parallel to the lines of force

The only terms of the expansion (9) which produce sensible currents in a rotating disk are those tessaral solid harmonics for which $n-s$ is odd The induced current-function is found to be (taking, say, $\cos s\omega$ in (9))

$$\phi = -\frac{2\,.\,4\,\ldots\ldots\,(n+s-1)}{1\,.\,3\,\ldots\ldots\,(n-s)}\,\frac{\bar{\mathrm{A}}}{\pi^2}\sin\eta\,(1-\mu^2)^{\frac{1}{2}s}\frac{d^s\mathrm{P}_n(\mu)}{d\mu^s}\sin(s\omega-\eta), \qquad (10)$$

where $\eta = \text{arc tan } sp\tau,$

τ having the value (7)

The most important type of induced currents is when $n = 2$, $s = 1$, in which case

$$\bar{\Omega} \propto xz,$$

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* See Ferrers, 'Spherical Harmonics,' chap vi

so that the lines of force at the disk are normal to it, but the direction of the force is reversed as we cross the axis of y The current function relatively to axes displaced through the proper angle η in the direction of rotation, varies as

$$y\sqrt{\{1-r^2/a^2\}}$$

A drawing of the current lines for this case is given As already mentioned they are simply the orthogonal projections of the contour lines of the tessaral harmonic of the second order

In the next type we have $n = 3$ $s = 2$, so that

$$\bar{\Omega} \propto z(x^2-y^2),$$

and the current function, relatively to displaced axes as before varies as

$$xy\sqrt{\{1-r^2/a^2\}}$$

IV "On the Magnetisation of Iron in Strong Fields" By Professor J A Ewing B Sc, F R S E University College, Dundee, and Mr William Low Communicated by Sir W Thomson, Knt, LL D, F R S Received March 2, 1887.

(Plate 2)

The behaviour of iron and steel when subjected to very strong magnetising forces is a matter of considerable practical and very great theoretical interest especially from its bearing on the molecular theory of magnetisation, which assigns an upper limit to the intensity of magnetism that a piece of iron can acquire and even suggests that the metal may become diamagnetic under the influence of a sufficiently great force All experiments hitherto made by magnetising iron in the field of an electric solenoid, have shown that the intensity of magnetism $\mathfrak{I}$, as well as the induction $\mathfrak{B}$, is increasing with the highest values actually given to the magnetising force $\mathfrak{H}$ It is scarcely practicable, however, to produce by the direct action of a magnetising solenoid, a field whose force exceeds a few hundreds of C G S units

To refer to a few recent experiments of this class —In experiments by one of us* on the magnetisation of long wires the highest value of $\mathfrak{H}$ applied to iron was about 90, and this gave an induction $\mathfrak{B}$ of 16,500 in a soft iron wire In Dr Hopkinson's experiments† a force of 240 gave 19,840 for the induction in a bar of mild Whitworth steel, and 18 250 in a bar of wrought iron.* The corresponding values of $\mathfrak{I}$ are 1563 and 1437 respectively. Probably the highest magnetisation reached in any experiments of this class already published is that found by Mr Shelford Bidwell† in his experiments on the tractive force between the halves of a divided ring electro magnet. For a force $\mathfrak{H}$ of 585 he gives 19 820 as the value of $\mathfrak{B}$ (calculated from the tractive force) in a wrought iron ring. The corresponding value of $\mathfrak{I}$ is 1530.

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* Ewing 'Exp Res in Magnetism' 'Phil Trans' 1885 Part II

† J Hopkinson, 'Magnetisation of Iron," Phil Trans, 1885, Part II

With cast iron Dr Hopkinson found (in a sample of grey iron) 10,783 for the induction produced by a force of 240. The corresponding value of $\mathfrak{I}$ is 841.

In the space between the pole pieces of a strong electro magnet we have a field of force of much greater intensity than it is practicable to produce by the direct action of the electric current. This field is not well adapted for experiments whose object is to determine with precision the relation of magnetisation to magnetising force on account of the distortion which it undergoes when the piece of iron to be magnetised is introduced into it. It is however well suited for experiments whose object is to determine how much magnetism the metal can be forced to take up.

For this purpose it is of course necessary that the cross section of the test-piece should be much smaller than the area of the pole piece faces. In the following experiments the electro magnet consisted of a pair of vertical limbs 25 cm long, with cores 5 cm in diameter, joined at the bottom by a horizontal yoke, and furnished on the top with pole pieces made of soft hammered scrap iron, in the form of rectangular blocks with plane faces whose distance from each other could be adjusted at will. The faces were 5·25 cm square. The magnet was wound with wire large enough to permit a current of about 27 amperes to be used for a short time. In the earliest experiments the test piece to be magnetised was a round cylinder of soft iron, with flat ends 0·34 cm in diameter and 1·3 cm long. This was covered with an induction coil, consisting of a single layer of fine wire which extended over the whole length of the piece. It was placed lengthwise in the centre of the field, with the pole pieces just touching its ends, and the field magnet was excited. The test piece was then suddenly withdrawn, while the transient current produced in the induction coil was measured by a ballistic galvanometer connected to the induction coil by long leading wires, which were twisted together throughout their whole length Very few experiments were made with test pieces of this form, for it was found that they gave by no means an exceptionally high value for the magnetic induction This is to be ascribed to the fact that the ends of the cylinder, which were in contact with the pole pieces necessarily shared that value of the induction which existed in the part of the pole piece faces which they touched and this comparatively low induction in and near the ends of the cylinder neutralised the much higher value in the middle portion The induction coil being wound from end to end of the bar gave a mean value for the whole length To obtain higher values, it was obviously necessary to restrict the measurement of the induction to the middle portion where the induction was greatest and further it was desirable to furnish the bar with conical or some form of spreading ends which would present an easy path for the lines of induction to converge towards the central neck Accordingly test pieces were turned of the form and dimensions of Sample A shown in Plate 2 fig 1, where the bobbin is sketched in place between the pole pieces These were wound along the whole length of the narrow central neck with an induction coil consisting of a single layer of No 36 S W G silk covered wire In Sample A the diameter of the iron neck was 0 923 mm and the diameter measured to the middle of the thickness of the wire forming the induction coil was 0 9435 Hence there was but little space, outside the section of the iron enclosed by the coil and the small amount of magnetic induction in this non ferrous space was allowed for by a method to be explained below

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* J and E Hopkinson have observed an induction of 20 000 in the core of a dynamo armature under a force estimated at 740 ('Phil Trans,' 1886 (Part I) p 355).

† S Bidwell 'On the Lifting Power of Electro magnets and the Magnetisation of Iron,' Roy Soc Proc, vol 40, 1886, p 486.

In test pieces of the form of Sample A the loss of magnetism observed on suddenly withdrawing the piece from its place between the pole pieces of the field magnet, is less than the whole magnetism by the small but somewhat uncertain quantity of residual magnetism which the piece retains To avoid this source of uncertainty another form of test piece was used, which is shown in fig 2, Sample B Here the bobbin has its conical ends rounded at the base to form portions of a circular cylinder, and the pole pieces are hollowed to correspond The bobbin can now be turned completely round about a central axis at right angles to the paper, so that the direction of its magnetism is reversed, and half the ballistic effect of the reversal measures the magnetic induction This method was used in the greater number of the observations Again, by merely withdrawing the bobbin from the field, and comparing the effect of this withdrawal with half the effect of reversal, an estimate was arrived at of the amount of error to which the former experiments were subject on account of residual magnetism

To determine the intensity of the magnetic field in the space immediately surrounding the narrow neck in which the greatest induction occurred, a small quantity of wire was wound over the first induction coil, to form a distance piece and on the top of that a second induction coil was wound the second coil like the first, consisting of a single layer of very fine wire The space between the two coils was accurately determined When the test piece was reversed or drawn out of the field the operation was in each case performed several times, and two groups of observations were recorded one giving the induction in the inner coil and the other the induction in the outer coil the difference of course served to determine the field in the space between the coils When this field was known it was easy to correct for the induction in the non ferrous space enclosed by the inner coil

Three kinds of wrought iron were tested, soft hammered scrap Swedish iron, and Lowmoor iron The hammered scrap proved less susceptible than the other two, and was not used in the final experiments which were made with test-pieces of the form of Sample B Pieces of cast iron were also tested in forms resembling both A and B

To determine in absolute measure the value of the ballistic effects, a large earth coil was kept in circuit with the induction coil and galvanometer, and was turned over in either the vertical or horizontal earth field at the beginning and again at the end of each group of observations To avoid the possibility of error in this important particular, two separate earth coils of entirely different dimensions were employed, and the galvanometer constant was determined independently by means of both with results which were in excellent agreement The values of the induction stated below are worked out on the basis that the horizontal force in the grounds of University College Dundee, at a place sufficiently removed from local magnetic influence, is 0 160 in C G S units

The following experiments are representative of a considerably larger number —

Lowmoor iron, annealed before turning the bobbin from a forged bar Sample B of shape and dimensions shown in fig 2 Diameter of iron neck = 0 65 cm , length = 0 44 cm Diameter to middle of inner induction coil, 0 6765 cm Diameter to middle of outer induction coil, 0 9364 cm

Area of section of iron (S_1) = 0 3318 sq cm

Area of space to be corrected for under inner induction coil (S_2) = 0·0276 sq cm

Area of space between inner and outer coil (S_3) = 0 3293 sq cm

Number of turns on inner induction coil = 16, number on outer coil = 12

In the following table D_1 is the throw of the ballistic galvanometer given by the inner coil when the test-piece was turned round, and D_2 is the throw given by the outer coil X_1 and X_2 are the corresponding *total* inductions in C G S units The difference of these, given in the fifth column, when divided by S_2, is the intensity of field or magnetic force per sq cm in the space immediately surrounding the iron This is given in column VI Multiplying it by S_2 we have the correction to be subtracted from X_1, which is given in column VII Finally, by dividing the corrected value of X_1 by the section of the iron S_1, we find 𝔅 the magnetic induction in the iron per sq cm Column IX gives the current in the field magnet coils in ampères

Lowmoor Wrought Iron Sample B

I D_1	II X_1	III D_2	IV X_2	V X_2-X_1	VI Field round iron neck per sq cm	VII Correction to be subtracted from X_1	VIII 𝔅	IX Current in field magnets ampères
127	8 295	109	9 490	1195	3 630	100	24 700	1 98
143	9 340	132½	11 540	2200	6 680	180	27 610	4 04
150	9 800	142	12 370	2570	7 800	220	28 870	5 81
153	9 990	148	12 890	2900	8 810	250	29 350	7 60
157½	10 280	154	13 410	3130	9 500	260	30 200	11 0
160	10 450	157	13 670	3220	9 780	270	30 680	13 5
161	10 520	160	13 930	3410	10 360	290	30 830	16 2
164	10 710	164	14 280	3570	10 840	300	31 370	21 6
165	10 780	166	14 460	3680	11 180	310	31 560	26 8

In another test of Lowmoor iron conducted in the same way, a still higher value of 𝔅 was reached namely, 32 880 This is the highest induction that has been recorded in these experiments

A similar experiment with a piece of Swedish wrought iron, of the form and dimensions shown in fig 2, gave 32,310 for the greatest value of 𝔅 the magnetic force in the ring of space surrounding the iron neck being then 11,250

The amount of residual magnetism retained by a Lowmoor sample of this form (Sample B) was determined by comparing the effect of withdrawing the test piece with the effect of reversing it The results showed that within the range of magnetic force used in these experiments, namely, from about 4000 to 11 000 C G S units, the residual magnetism is nearly constant Its mean value in a number of determinations was—

For Lowmoor iron, residual induction, $𝔅_r$ = 510 per sq cm

For Swedish iron, residual induction, $𝔅_r$ = 500 per sq cm

These results showed that pieces of the form of Sample B (fig 2) retained only a small part (less than 1/60) of their greatest induction when withdrawn from the field The proportion of residual to greatest induced magnetism in samples of the form A (fig 1), is probably not very different from this

In the following experiment a bobbin of annealed Swedish iron, of the size and shape shown in fig 2 was tested by withdrawing it from the field The columns of the table have the same meaning as before except that the quantity in column VIII now headed $\mathfrak{B} - \mathfrak{B}_r$ is not the whole induction per sq cm but that part of the induction which disappeared when the test piece was withdrawn from the field In this case the section of the iron was the same as before but the space between the inner and outer induction coils (S_3) was 0 308 sq cm There were fourteen turns in the inner coil and twelve in the outer

Swedish Wrought Iron Sample B

I	II	III	IV	V	VI	VII	VIII	IX
D	X_1	D_2	X_2	$X_2 - X$	Field round iron neck per sq cm	Correction to be subtracted from X	$\mathfrak{B} - \mathfrak{B}_r$	Current in field magnets in ampères
125 5	9 290	131 5	11 350	2060	6 690	180	27 450	4 08
134 5	9 950	147 0	12 690	2740	8 900	250	29 230	7 77
138 5	10 320	153 5	13 250	2930	9 510	250	30 320	10 9
141 5	10 470	157 0	13 550	3080	10 000	280	30 710	14 2
143 5	10 620	160 0	13 810	3190	10 300	290	31 130	16 5
144 0	10 660	162 0	13 990	3330	10 810	300	31 [illegible]20	18 3
145 5	10 770	163 5	14 120	3350	10 880	300	31 [illegible]	22 9
147 0	10 880	166 0	14 330	3450	11 200	310	31 860	26 5

The residual magnetism may be corrected for by adding 500 as the value of $\mathfrak{B}_r$ to each of the numbers in column VIII We then obtain for the highest induction $\mathfrak{B}$ the value 32 360

The following results relate to test pieces of the form and size shown in fig 1 —

Swedish wrought iron Form of Sample A Section of iron neck = 0 669 sq cm Section to middle of induction coil = 0 708 sq cm Loss of induction per sq cm tested on withdrawing the bobbin ($\mathfrak{B} - \mathfrak{B}_r$)

Current in field magnets ampères	$\mathfrak{B} - \mathfrak{B}_r$
3 92	27 550
7 48	29 420
11 3	30 240
14 0	30 460
17 9	30 960
20 1	31 180
20 4	31 290

These figures agree very well with those in the preceding table, which related to another sample of different form cut from the same bar. Probably 500 is in this case also a fair estimate of the residual induction, and by adding that to the values given above we arrive at probable values of $\mathfrak{B}$.

Lowmoor wrought iron. Form of Sample A. Dimensions as above. Current in field magnets = 20.4 amperes. Loss of induction on withdrawing the bobbin $(\mathfrak{B} - \mathfrak{B})$ = 31 660. Allowing for the residual magnetism, this gives an induction exceeding 32,000.

Soft Hammered Scrap. Form of Sample A. Dimensions as above.

Current in field magnets	$\mathfrak{B} - \mathfrak{B}_r$
20.4	31 230
26.2	31,520

The remaining experiments relate to cast iron. The following results are for a sample of the form shown in fig. 2, except that the neck was of considerably larger diameter, namely 0.962 cm. The sample was tested by turning it end for end in the magnetic field.

Section of neck = 0.727 sq. cm.

Section within middle of inner induction coil = 0.767 sq. cm.

Space to be corrected for = 0.040 sq. cm.

Section within middle of outer induction coil = 1.195 sq. cm.

Space between coils = 0.328 sq. cm.

Cast Iron

X_1	X_2	$X_1 - X_2$	Field round iron neck per sq. cm.	Correction to be subtracted from X_1	$\mathfrak{B}$	Current in field magnets amperes
14 450	15 730	1280	3 900	160	19 160	1.97
16 200	18 300	2100	6 400	260	21 930	3.75
16 910	19 440	2530	7 710	310	22 830	5.38
17 420	20 070	2650	8 080	320	23 520	7.08
18 240	21 260	3020	9 210	370	24 580	13.15
18 490	21 670	3180	9 700	390	24 900	16.9
19 030	22 510	3480	10 610	420	25 600	22.6

Another set of readings were taken with this sample at the same time, by drawing it suddenly out of the field in order to determine the residual induction. The results showed that throughout the range of magnetic forces employed here the residual induction had a nearly constant value of 400 C.G.S. units per sq. cm.

A bobbin of cast iron of a form resembling Sample A, fig. 1, was also tested by drawing it out of the field The results were in close agreement with those given above for the other sample

In fig 3 the general results for Lowmoor wrought iron (Sample B) and cast iron are shown by curves which give the relation (1) of the induction $\mathfrak{B}$ within the metal neck to the current in the field magnet coils, and (2) of the induction or magnetic force in the space immediately surrounding the neck to the current in the field magnet coils The full lines are for the Lowmoor forging and the broken lines are for cast iron The field produced by a given current is (at its higher values) rather less strong in the case of cast iron, probably because the larger size of the cast iron neck allowed a greater portion of the whole induction from pole to pole to find its way through the metal (Compare X_1 for cast iron and for Lowmoor)

The magnetic force within the metal ($\mathfrak{H}$) differs from the field in the surrounding space by an amount which cannot be estimated without a knowledge of the distribution of free magnetism on the pole-pieces and conical faces of the bobbin It appears probable that with the dimensions of the various parts used in these experiments, the magnetic force within the metal is less, but not very greatly less, than the outside and closely neighbouring field In the absence of any exact knowledge of $\mathfrak{H}$ it is interesting to examine the relation of $\mathfrak{B}$ to the outside field Thus, ($\mathfrak{B}$—outside field)/4π gives a quantity which is probably not much less than the intensity of magnetism $\mathfrak{I}$ The values of this quantity for Lowmoor wrought iron Swedish wrought iron, and cast iron are stated below In the case of the Swedish iron the values of $\mathfrak{B}-\mathfrak{B}_r$ given in the previous table for that metal have had 500 added to allow for the residual magnetism Again, the quantity $\mathfrak{B}$/outside field is probably not much less than the magnetic permeability μ its values also are given below

I Lowmoor Wrought Iron

Outside field	$\mathfrak{B}$	$\frac{\mathfrak{B}-\text{outside field}}{4\pi}$	$\frac{\mathfrak{B}}{\text{outside field}}$
3 630	24 700	1680	6 80
6 680	27 610	1670	4 13
7 800	28 870	1680	3 70
8 810	29 350	1680	3 33
9 500	30 200	1650	3 18
9 780	30 680	1660	3 14
10 360	30 830	1630	2 98
10 840	31 370	1630	2 89
11 180	31 560	1620	2 82

II Swedish Wrought Iron

Outside field	$\mathfrak{B}$	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$	$\frac{\mathfrak{B}}{\text{outside field}}$
6 690	27 960	1700	4 18
8 900	29 730	1660	3 34
510	30 820	1700	3 24
10 000	31 210	1690	3 12
10 360	31 630	1700	3 05
10 810	31 720	1670	2 94
10 880	32 060	1680	2 95
11 200	32 360	1690	2 90

III Cast Iron

Outside field	$\mathfrak{B}$	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$	$\frac{\mathfrak{B}}{\text{outside field}}$
3 900	19 660	1250	5 04
6 400	21 980	1240	3 42
7 710	22 890	1200	2 96
8 080	23 520	1230	2 91
9 210	24 580	1220	2 67
9 700	24 900	1210	2 57
10 610	25 600	1190	2 46

Fig 4 shows by curves the relation of $\mathfrak{B}$ to $\mathfrak{B}$/outside field for Lowmoor iron and for cast iron in the manner introduced by Rowland for showing the relation of $\mathfrak{B}$ to μ The curves have the same kind of inflection that a curve of μ and $\mathfrak{B}$ begins to have when the magnetising force is raised sufficiently high * The range through which the permeability of iron may vary is well shown by comparing the values reached here (probably in the extreme case less than 3) with the value 20 000 which was found by one of us in the case of a soft wire exposed to a very small magnetising force and kept at the same time in a state of mechanical vibration †

The quantity ($\mathfrak{B}$ — outside field)/4π is nearly constant in the Swedish iron but diminishes with increased induction in the Lowmoor iron and in the cast iron If the outside field were an accurate measure of

---

* This feature of the curve of μ and $\mathfrak{B}$ was not noticed by Rowland himself who applied to his curve an empirical formula which fails to take account of it It has however been noticed by several later observers (Fromme Wiedemann Annalen vol 13 p 695 Ewing *loc cit* p 574 Bidwell *loc cit* p 495)

† Ewing *loc cit* p 567

$\mathfrak{H}$, this would mean that in the two metals last named $\mathfrak{I}$ had passed a maximum, and the process of diamagnetisation which the Ampère-Weber molecular theory of magnetism anticipates had set in But the uncertainty which attaches to the value of $\mathfrak{H}$ prevents this conclusion from being fairly drawn from these experiments A slight excess in the mean value of $\mathfrak{H}$ within the metal neck over the value of $\mathfrak{H}$ in the space contiguous to the neck would suffice to convert the apparent decrease of $\mathfrak{I}$ into an increase, with increasing values of $\mathfrak{H}$ So far as these results can be said to bear upon the point in question, they rather support the idea that the intensity of magnetism $\mathfrak{I}$ becomes and remains a sensibly constant quantity when the magnetising force is raised to very high values This maximum of $\mathfrak{I}$ appears to exceed 1700 in wrought iron and 1250 in cast iron, and it does not appear likely that any increase of magnetising force will bring the intensity of magnetism in cast iron to a value equal or nearly equal to that which wrought iron is capable of acquiring It is scarcely necessary to add that our experiments give no support to the suggestion that there is a maximum of the induction $\mathfrak{B}$ The value of $\mathfrak{B}$ capable of being reached by the method we have employed depends mainly on the scale of the experiments Larger field magnets with pole pieces tapering to a narrow neck should yield values of $\mathfrak{B}$ greatly in excess even of those we have observed

The experiments will be continued and various qualities of steel will be examined with the following modification in the apparatus —The pole pieces will themselves be turned, at the ends which face each other, into cones with flat ends, between which the test piece in the form of a round cylinder will be inserted The induction will be measured in the neighbourhood of a medial transverse plane only, and the value of the field outside the iron will be determined in this plane at various distances from the axis Since there is no free magnetism in the iron bar in the medial plane, the magnetic force within the metal is continuous with the force in the surrounding space, and a curve showing the relation of the magnetic force at various points outside to the distance from the axis should admit of being produced so as to give a good approximation to the magnetic force within the metal If this can be successfully accomplished, the value of the *isthmus* method of examining the magnetisation of iron will be greatly enhanced

[Dr Hopkinson informs me that he experimented by what we have called the "isthmus" method nearly three years ago, but gave it up from uncertainty about the induction which took place through the coil but not through the iron In the present experiments this difficulty has been avoided mainly by using larger bobbins with a single layer of fine wire for induction coil I am indebted to Dr Hopkinson for the suggestion (soon to be put in practice) that the "isthmus" method should be applied to the manganese steel whose non-magnetic quality under ordinary conditions has been already commented on by himself as well as by Mr. J. T. Bottomley and Professor Barrett. In connexion with the values of $\mathfrak{B}$ reached by other observers, Professor J. J. Thomson informs me that in some recent experiments by himself and Mr. H. F. Newall on the effect of cutting a magnet at right angles to the lines of force, an induction of 28,000 was found on one occasion.—J. A. E.]

Presents, March 24, 1887.

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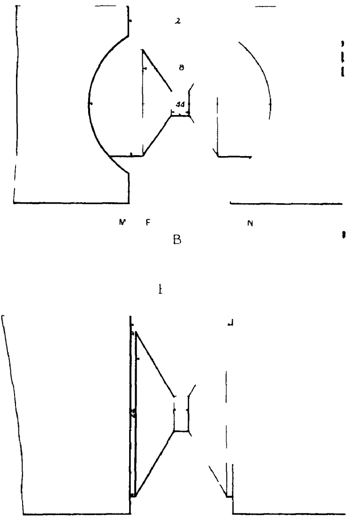

March 31, 1887

Professor STOKES, D C L, President, in the Chair

The Presents received were laid on the table, and thanks ordered for them

The following Papers were read —

I. "Note on the Development of Voltaic Electricity by Atmospheric Oxidation' By C R ALDER WRIGHT, D Sc, F R S Lecturer on Chemistry and Physics, and C THOMPSON, F C S, Demonstrator of Chemistry in St Mary's Hospital Medical School Received March 10, 1887

It is well known that when metallic copper is brought into contact simultaneously with atmospheric air and aqueous solution of ammonia, rapid oxidation is set up, the copper oxide formed dissolving in the liquid, producing a *blue* solution of ammoniacal cupric oxide, or cuprammonium hydroxide Whilst investigating processes for the manufacture of this fluid (now used commercially on a considerable scale) we noticed that if the air supply be greatly in deficiency relatively to the bulk of the copper under certain conditions the solution is but little coloured, containing copper dissolved principally as cuprous, and not as cupric oxide This might perhaps, be anticipated *à priori* inasmuch as it is well known that blue cupric solution in ammonia, when digested with metallic copper in the absence of air, takes up a second equivalent of copper, becoming colourless cuprous solution, but further experiments seem to indicate that the production of cuprous oxide under the oxidising influence of a limited supply of air is the primary action, and not merely a secondary result, in short, that the first step in the change is expressed by the reaction—

$$4Cu+O_2 = 2Cu_2O,$$

cuprous oxide being formed, which then (under favourable conditions) becomes further oxidised to cupric oxide, thus—

$$2Cu_2O+O_2 = 4CuO,$$

and not by the reaction—

$$2Cu+O_2 = 2CuO,$$

the cupric oxide thus formed as the first product becoming subsequently reduced (in the absence of air) to cuprous oxide, thus—

$$\mathrm{CuO + Cu = Cu_2O}$$

When a sheet of copper is kept out of direct contact with air by being immersed in ammonia solution, oxidation of the metal is gradually effected by virtue of the dissolving of oxygen from the air at the surface of the fluid, and diffusion of the oxygen solution to the vicinity of the copper. This action is an extremely slow one if the copper be covered by some depth of fluid and if the setting up of convection currents through heating or evaporation be prevented by keeping the vessel perfectly at rest and at an equable temperature, and well closed to prevent escape of ammonia; but if these precautions be neglected it goes on much more rapidly, and the liquid comparatively soon becomes blue; it can, however, be also materially accelerated by arranging horizontally on the surface of the fluid a plate of platinum or other electrically conducting material not chemically acted upon by the fluid, and connecting this by means of a wire, &c., with the copper plate. The upper conductor, or *aeration plate* as it may be conveniently termed, being simultaneously in contact with the atmosphere and fluid, attracts to its surface a film or aura of condensed gases, the oxygen of which becomes gradually transferred to the copper, a voltaic current circulating through the fluid and connecting wire. Cuprous, and not cupric, oxide thus results, dissolved in the ammonia solution in contact with the copper plate: the mechanism of the reaction being conveniently represented by the scheme—

$$\left\{\begin{array}{l}\text{Copper plate}\quad \mathrm{Cu_2} \mid \mathrm{OH_2} \mid \mathrm{OH_2} \mid \mathrm{O}\quad \text{Aeration plate,}\\ \text{Copper plate} \mid \mathrm{Cu_2O} \mid \mathrm{H_2O} \mid \mathrm{H_2O} \mid \quad \text{Aeration plate,}\end{array}\right.$$

water being represented as the electrolyte for simplicity's sake. The air film on the aeration plate being constantly renewed by absorption from the atmosphere, the process goes on continuously as long as the two plates are connected together by the wire. This wire may be lengthened at will so as to make the current which passes through it whilst the action goes on relatively stronger or weaker according to the amount of resistance introduced into the circuit; and by including a galvanometer or silver voltameter in the circuit the ordinary phenomena due to the passage of currents are readily recognisable.

A voltaic cell thus produced "runs down" very rapidly when the resistance in circuit is diminished, more or less recovering when the resistance is again increased; with a large resistance (*e.g.*, sufficient to reduce the current density to a micro ampere or less per square centimetre of aeration plate surface), a very notable E.M.F. is maintained, amounting under favourable conditions to 0.5 or 0.6 volt The maximum E M F thus capable of development varies considerably with the strength of the ammoniacal solution, being the less the weaker the fluid, addition of common salt or of sal ammoniac to the liquid notably increases the E M F and diminishes the internal resistance of the cell Spongy platinum in a thin layer as the aeration plate gives higher values than thin platinum foil, the highest numbers thus obtained, using pretty concentrated ammoniacal brine, fell but little short of 0.8 volt, or somewhat less than the E M F corresponding with the heat of formation of cuprous oxide,* since, according Julius Thomsen, $Cu_2,O = 40810$ = about 0.88 volt

It is obvious that this copper atmospheric oxidation cell has a close connexion with the "air battery" described in 1873 by Gladstone and Tribe ('Roy Soc Proc,' vol 21, p 247) in which what is virtually an "aeration plate,' consisting of a tray full of crystals of silver is used, opposed to a copper plate immersed in a solution of copper nitrate Cuprous oxide is formed in both cases, in virtue of the indirect combination brought about between the oxygen of the air and the copper but there is this great difference between the two (apart from the cuprous oxide being deposited as such in Gladstone and Tribe's arrangement, and being kept in solution in ours), that in the one the cuprous oxide is formed *at the surface of the copper plate* itself, and in the other *at the surface of the aeration plate* This essential difference is embodied in the above depicted scheme as compared with the following one which represents the action in Gladstone and Tribe's cell —

$$\left\{\begin{array}{l} \text{Silver}+O \;\Big|\; \begin{matrix} Cu(NO_3)_2 \\ Cu(NO_3)_2 \end{matrix} \;\Big|\; \begin{matrix} Cu(NO_3)_2 \\ Cu(NO_3)_2 \end{matrix} \;\Big|\; \left.\begin{matrix} Cu \\ Cu \end{matrix}\right\} \text{Copper} \\[2ex] \text{Silver} \;\Big|\; O \left\{\begin{matrix} Cu \\ Cu \end{matrix}\right. \;\Big|\; \begin{matrix} (NO_3)_2Cu \\ (NO_3)_2Cu \end{matrix} \;\Big|\; \begin{matrix} (NO_3)_2Cu \\ (NO_3)_2Cu \end{matrix} \;\Big|\; \text{Copper} \end{array}\right.$$

One result of this difference is that the surface of the aeration plate in the ammonia cell is kept constantly the same, whereas in the nitrate cell it is continually changing its character through deposition of solid cuprous oxide on the silver in consequence of this deposition, whilst the E M F. of the ammonia cell, *cæteris paribus*, is constant, that of the nitrate cell is continually varying Gladstone and Tribe, moreover, only obtained an E M F of $\frac{8}{83}$ to $\frac{11}{83}$ of a Daniell, or about 0.104 to 0.143 volt, even under the most favourable conditions, viz, when the cell was connected with an electrometer, whilst four or five times this amount is indicated by the cells examined by us

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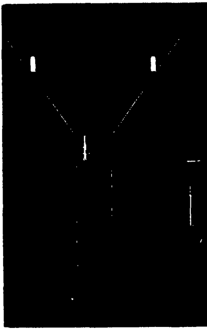
* The actual chemical change going on in the cell is the synthesis of cuprosammonium hydroxide, so that the (unknown) heat of solution of cuprous oxide in ammonia should be added to this to obtain the total heat development



In order to examine separately the fluids collecting round the two plates after action had gone on for some time we employed cells of U shape, and to obtain as large an aeration surface as possible, we adapted to one leg of the U a funnel (as indicated in the figure) with the stem cut off and united to the U tube by a piece of india-rubber tubing *a*, slipped tightly over the junction. The other end of the U tube was closed with an india rubber cork *b* through which passed a piece of glass tubing with a platinum wire, *c*, sealed into it at the lower end and filled with mercury, thus forming a mercury cup, and serving to make contact with the copper plate, *d*, which was soldered to the end of the platinum wire, the soldering and platinum being coated with gutta percha, so that only the copper plate was in contact with the fluid with which the U tube was subsequently filled. A similar glass tube and platinum wire mercury cup, *f*, served to make contact with the aeration plate, which was conveniently supported horizontally at the surface of the fluid in the funnel by means of a disk of porous earthenware, *e*. By fixing a rim of gutta percha round this disk so as to convert it into a sort of tray like the lid of a pill box, and filling this tray with platinum sponge, an aeration plate of spongy metal was readily obtained. By interposing suitable re- s, galvanometer, silver voltameter, &c, in the external circuit obtained on connecting the two mercury cups by a wire, the current passing could be modified at will, and shown to exhibit all the ordinary phenomena of moderately weak currents

After continued action with small resistance only in circuit, the liquid in the funnel was found on analysis to contain no copper what ever, whilst that surrounding the copper plate, though colourless before removal from the tube, speedily became blue on exposure to air and contained more or less considerable amounts of copper in solution, obviously originally in the condition of *cuprous* oxide, Cu_2O

Following up the ideas suggested by the above observations, we are making a number of experiments with a variety of analogous combinations, in which atmospheric oxidation constitutes the essential chemical action taking place, by varying the nature of the aeration plates, the metals dissolved, and the liquids employed (as also by substituting other gases, *e g*, chlorine, for air), a large number of combinations are obviously obtainable Some of those which we have so far examined present points of considerable interest, the oxidising action exerted under favourable conditions being strongly marked, so much so that certain metals, *e g*, mercury and silver, not ordinarily prone to atmospheric oxidation, can under suitable conditions be gradually oxidised and dissolved in appropriate liquids, just as the copper is dissolved in the ammonia in the cell above described, these actions, moreover, being accompanied by the development of currents of strength sufficient to cause measureable amounts of electrolytic decomposition outside the cell, *e g*, in a silver voltameter

II "Clausius's Formula for the Change of State from Liquid to Gas applied to Messrs Ramsay and Young's Observations on Alcohol" By Geo Fras Fitzgerald, M A, F T C D, F R S Erasmus Smith's Professor of Natural and Experimental Philosophy in the University of Dublin Received March 14, 1887

Clausius, in Wiedemann's 'Annalen,' vol 14, 1881, pp 279—290, and 'Phil Mag,' vol 12, 1881, p 381, and vol 13, 1882, p 132, has given an empirical formula for calculating the relation between the volume, pressure, and temperature of a substance in both liquid and gaseous states The equation he gives is a continuous one for an isothermal, and he determines the pressure at which evaporation takes place by considering that the work done in the transformation from liquid to gas at a constant pressure must be equal to what would be done if the transformation took place along the continuous isothermal He requires, for convenience in applying this to actual cases, to calculate the values of certain rather complicated exponential functions, and has published tables of their values which greatly facilitate the work of comparing his formula with observations of vapour-pressures at different temperatures He has compared his formula with determinations of vapour-pressure, &c, by Andrews and Regnault of carbon dioxide, and by Regnault and Sajotschewsky of ether, and with Regnault's experiments on water, and has shown that they agree very well He has also, by help of his formula, calculated the critical temperature for water, and finds it to be about 332° C, and the critical pressure to be 134 atmospheres

Professor Ramsay has kindly furnished me with his and Mr Young's observations on alcohol, and I have compared them with Clausius's formula, with which they agree very well

The formula Clausius has given may be described as follows —

The relation connecting the volume, pressure, and temperature of a substance can be expressed by the formula—

$$\frac{p}{\mathrm{RT}} = \frac{1}{v-\alpha} - \frac{1}{\Theta(v+\beta)^2}$$

In this R, α, and β are constants for each substance, p is the pressure, v the specific volume, T the absolute temperature and Θ is a function of the temperature which vanishes with T, and for which Clausius has given the formula—

$$^{*}\frac{\Theta_c}{\Theta} = (1+b)\left(\frac{\mathrm{T}_c}{\mathrm{T}}\right)^n - b$$

in which b and n are constants for any one substance, and T_c and Θ_c the values of T and Θ at the critical temperature

From a consideration of the isothermals represented by Clausius's formula it is easy to show that the critical isothermal, for which two of the tangents parallel to the axis from which pressures are measured coincide, gives—

$$\Theta_c = \frac{8}{27(\alpha+\beta)}$$

As the combination $\alpha + \beta$ occurs frequently, Clausius denotes it by γ He expresses the specific volumes of the saturated liquid and gas by σ and s, and uses w and W for $\sigma - \alpha$ and $s - \alpha$ respectively He also uses the symbol $\Pi = \mathrm{P}/(\mathrm{RT})$ where P is the saturated vapour-pressure, and subscribes c, thus Π_c, to express the value of any of these quantities at the critical point Hence we get—

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* There is a misprint of $\frac{\Theta}{\Theta_c}$ for $\frac{\Theta_c}{\Theta}$ on the last line of the text, formula 7, of p 185 of Clausius's second paper in the 'Phil Mag vol 18

$$\Pi_c = \frac{1}{8\gamma} \qquad\qquad W_c = w_c = 2\gamma.$$

I have assumed as the result of observation that $T_c = 516{\cdot}5$ and $P_c = 49{,}000$ mm. This latter value is probably a little too large, but, as there seems some uncertainty as to its value from the experiments, from which it is very difficult to approximate accurately to the actual position of the critical point, I have thought this value sufficiently accurate. I have calculated the result of making changes in this value, and any variation within limits allowable by the experiments does not materially affect my results.

The values of these constants for alcohol as determined from the observations are—

$$\begin{aligned}
R &= 1351{\cdot}35 \\
T_c &= 516{\cdot}5 \\
P_c &= 49{,}000 \\
\alpha+\beta = \gamma &= 1{\cdot}780 \\
W_c = w_c = 2\gamma &= 3{\cdot}560 \\
\Pi_c = \frac{1}{8\gamma} &= 0{\cdot}07023 \\
\Theta_c = \frac{8}{27\gamma} &= 0{\cdot}1664 \\
b &= 0{\cdot}9118 \\
n &= 1{\cdot}3462
\end{aligned}$$

I have found that constant values for α and β do not satisfy the observations accurately, and that α varies from 1·087 at 0° C. to 0·184 at 240° C., as I explain further on. I calculated b and n so as to make the saturated vapour-tensions correct at 0° C. and 100° C. The process of calculation I adopted was to calculate Π/Π_c at 0° C. and 100° C., and then from Clausius's tables obtain the corresponding values of Θ/Θ_c. This gave two equations, to determine b and n by means of $\Theta_c/\Theta = (1 + b)(T_c/T)^n - b$. The equation for n being—

$$\frac{\dfrac{\Theta_c}{\Theta} - 1}{\dfrac{\Theta_c}{\Theta'} - 1} = \frac{\left(\dfrac{T_c}{T}\right)^n - 1}{\left(\dfrac{T_c}{T'}\right)^n - 1}.$$

This was solved by trial and error and then b calculated. Having determined b and n, the value of Θ/Θ_c for any temperature may be easily calculated, and thence the corresponding values of Π/Π_c, W/W_c, and w/w_c obtained by interpolation from Clausius's tables. From these P, W, and w are calculated and compared with observation. It is from this comparison of w with the volume of the liquid at various temperatures and pressures that it is evident that α is not constant, for the difference between w calculated and σ as observed is by no means so.

The following table exhibits some of my results:—

	0° C	100° C	200° C	240° C	242.5° C	243.5° C
P observed	12.24	1695	22434	46389	—	—
P calculated	12.24	1695	22106	46172		4[illegible]00
W	30.210	284.5	19.09	5.43	4.26	3.56
v	29.046	284.4	19.85	5.83	4.62	—
w	0.177	0.389	1.008	2.43	2.99	3.56
σ	1.264	1.390	1.796	2.614	2.925	—
$\sigma - w = \alpha$	1.087	1.001	0.788	0.184	−0.065	—

In some of these cases, as for example for W at 0° C., it is very difficult to interpolate accurately into Clausius's tables, and similarly for the values of w near the critical point, and I consequently do not attribute much accuracy to these values. On the whole, however, I think that, considering the enormous range of values to be represented by the formula, it is most remarkably accurate. When we compare the calculated and observed volumes of the liquid, in which case α is of importance, we find that no constant value for α can make them agree, for α obviously diminishes with increased temperature, and near the critical point the value of α for the liquid and gaseous states is not the same. All this means of course that Clausius's formula does not apply accurately to the case of alcohol. Clausius has not, as far as I can find, applied his formula to calculate the volumes of liquids, and without doing so the want of constancy in α would not appreciably affect the result. Messrs. Ramsay and Young have made observations of the volume of the liquid at various temperatures and pressures, and I have compared some of their results with the formula. In this way it can be seen that α must be made a function of the pressure as well as of the temperature. I have calculated the values of $v - \alpha$ at certain temperatures and pressures, and find at 110° C.—

Pressure	2862	60 000
$v - a$ (calculated)	0 409	0 383
v (observed)	1 417	1 3925
a	1 008	1 0095

At 200° C —

Pressure	22 434	60 000
$v-a$ (calculated)	1 003	0 858
v (observed)	1 793	1 720
a	0 793	0 862

At 240° C —

Pressure	47 500	60 000
$v-a$ (calculated)	2 166	1 641
v (observed)	2 468	2 169
a	0 298	0 528

From this it is evident that a diminishes with increased temperature, and increases with increased pressure

Notwithstanding this, that Clausius's formula does not at all accurately represent the state of the liquid, there is no doubt that it gives a wonderfully accurate general representation of the more important features of the change of state In this respect it is of enormously higher value than the formulæ that only give the relations connecting the temperature and pressure of saturated vapours In addition to this, which it certainly gives in a rather complicated way, it gives the state of the liquid and gas before and after as well as during evaporation, and enables us to calculate points on the theoretical continuous isothermal connecting the liquid and gaseous states I have calculated enough of these points to roughly sketch in these curves that cannot be made the subjects of experimental investigation by the usual methods, owing to the instability of the states they

Clausius's equation may be put into the following form by assuming $p/(RT) = y$ and $v-\alpha = x$—

$$y(x+\gamma)^2 = (x+\gamma)^2 - \frac{x}{\Theta}$$

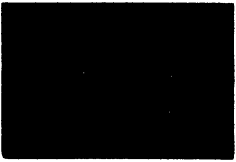
From this it is evident that an isothermal is a quartic curve having asymptotes $y = 0$ $x = 0$, and $x+\gamma$ a double asymptote at a cusp at infinity, so that the point at infinity on this line is a multiple point of a high order

If we calculate the positions of the points of tangency of tangents parallel to $y = 0$, for which consequently $dy/dx = 0$, we have the cubic equation—

$$(x+\gamma)^3 = \frac{2x^2}{\Theta},$$

and when two of its roots are equal $\Theta_c = \frac{8}{27\gamma}$ and this determines the critical isothermal The quartic consists of three branches One, a serpentine branch, lies in the positive region of x and mostly of y and is the only branch of physical interest at present The other two branches lie entirely in the negative region of x and y One of these is somewhat parabolic, and lies between $x = 0$ and $x = -\gamma$ asymptotic to both of them, the other is hyperbolic and asymptotic to $x = -\gamma$ and to $y = 0$ The always real solution of the cubic that determines the points where $dy/dx = 0$ for positive values of Θ and γ is a point on the parabolic branch of the curve that lies between $x = 0$ and $x = -\gamma$ The other two roots, when real, determine the highest and lowest points on the serpentine part of the curve that lies in the positive region of x The accompanying diagram represents the general

FIG 1



features of this class of curve It is the particular case of the quartic—

$$(L+aM)^2(M^2-LN)=bLM^3,$$

where L, M and N are lines in which M is the line infinity, and L and N are at right angles In the general case this quartic is a continuous curve with a cusp at the intersection of L and M and L + aM as the cuspidal tangent, while L is a tangent to another branch that passes through the same point Its general features are something like this—

Fig 2

General Feature Diagram

There is generally a double inflexion in the part of the curve outside the triangle L, M, N, and the particular one of a series of such curves for which the two inflexional tangents coincide is what corresponds to the critical isothermal in a gas It is only what corresponds to the part outside the triangle that is of physical interest

In the particular case of the curves representing the isothermals of alcohol, the negative parts of the curve lie at a very great distance

FIG 3.

from the origin compared with the dimensions of the serpentine part of the positive branch, so that it is not easy to represent them both in the same figure. I have calculated several points on the isothermals corresponding to 110° C., 207·5° C., and 243 5° C. 207·5° C. is the isothermal that just touches $y = 0$, while 243·5° C. is the critical isothermal. The dotted lines represent the saturated vapour-tensions for which the areas included in the loops of the curve above and below are equal. The points numbered 1, 2, 3, 4 are specially noticeable points on the curves so numbered in the general feature diagrams.

The isothermal 110° C. goes down at the point 1 entirely outside the diagram to a pressure of $-246{,}800$ mm, and intersects its vapour-pressure line outside the diagram to the right at a volume of 206. The negative parabolic branch of this isothermal only comes up at the point 3 to a pressure of $-4{,}442{,}000$ mm., while the corresponding points of 207 5° C and 243 5° C. come only to $-4{,}044{,}000$ mm. and $-2{,}743{,}000$ mm., so that they are very far off. In the isothermal 207·5° C. the point 4 lies at about $x = -32$, $p = -32{,}000$, so that it is not very far off.

What strikes me as most remarkable about these curves and other than what one might have expected, is the very great distance to which the point 1 descends at ordinary temperatures It would be interesting to compare the forms of these parts of the curve for several liquids, and see whether there was any connexion between it and the capillarity

III "The Influence of Stress and Strain on the Physical Properties of Matter. Part III Magnetic Induction." By HERBERT TOMLINSON, B A. Communicated by Professor W GRYLLS ADAMS, M A, F R.S. Received March 17, 1887.

(Abstract)

The author lays before the Society the results of experiments extending over a period of ten years on the effects of stress and strain on the magnetic permeabilities of iron, nickel, and cobalt

Two methods were employed In one the metal to be tested—usually in the form of wire—was placed with its axis coincident with that of a magnetising solenoid, in most cases of considerable length as compared with the diameter of the wire, round the central portion of the solenoid was wrapped a secondary coil A similar pair of primary and secondary coils, with a similar piece of the same specimen of metal, was balanced against the first by means of resistance coils, so that on closing the magnetising circuit no deflection was produced in a delicate Thomson's galvanometer suitably connected up with the resistance coils and secondary coils The alteration of magnetic permeability produced by stress was measured by the change necessary to be made in the resistance coils in order to restore the balance

In the second method the resistance coils were dispensed with, and only a metal core used in one of the two pairs of solenoids which were connected in series each to each The arrangements were such that the pairs of solenoids, when without any cores, balanced each other's effects on the galvanometer, so that the deflections of the latter instrument were due only to the magnetic permeability of the metal to be tested The alteration of permeability was in this case measured by the change of deflection produced in the Thomson's galvanometer The second method was the one principally employed

In all cases, where it is advisable, the results are either given in C G S units, or data are supplied for reducing to these units, moreover, the author has endeavoured to separate, as far as possible, the effects of stress on the *permanent* and on the *temporary* permeabilities of

the metals,* which effects are for the most part opposite in nature. The paper is illustrated by a large number of curves showing the relations between magnetic permeability and stress and strain at different temperatures from 0° C. to 300° C. and upwards. The information conveyed by these curves it is impossible to adequately represent in an abstract, but the following are among the chief conclusions arrived at:—

1. When there is no permanent load on an iron wire, and the magnetising force is small, longitudinal traction of small amount increases the temporary permeability. The increase reaches a maximum very quickly as the load increases, when further loading begins to diminish the magnetic permeability, until a certain limit has been reached, for which the permeability is a minimum. If the load be carried beyond the above limit the permeability begins to increase again with the load. As a consequence of the above, when the magnetising force is small there are *two* critical values of load for which the load produces no effect on the temporary permeability.

2. The first of the two critical values of loading mentioned above diminishes with increase of magnetising force, and finally vanishes when the latter reaches a certain limit.

On the contrary, the second critical value of loading increases with the magnetising force.

3. The maximum of temporary permeability mentioned in 1 diminishes as the magnetising force increases, and occurs at a less and less degree of loading until the latter begins to produce decrease instead of increase of permeability.

The minimum of temporary magnetic permeability, on the contrary, increases with the magnetising force, but like the maximum, occurs with a lower amount of load the higher the magnetising force.

4. The effects mentioned in 1, 2, and 3 as being produced by loading, are modified when a comparatively small load is left permanently on the wire. The modifications are stated in 5, 6, and 7.

5. For small magnetising forces loading produces no effect on the temporary magnetic permeability, unless carried beyond a certain limit. Beyond this limit further loading suddenly begins to increase the permeability.

6. For all values of the magnetising force the first critical value of loading vanishes.

The second critical value of loading increases with the magnetising force, but for a given magnetising force is much lower than when there is no permanent load.

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* By the terms permanent and temporary permeabilities are meant the permeability for permanent magnetisation, and the permeability for temporary magnetisation respectively.

7. The minimum temporary permeability increases with the magnetisingforce up to a certain limit of the latter, but beyond this limit decreases.

8. The permanent magnetic permeability is increased by loading, the amount of increase per cent. being very large for small magnetising forces and moderate loads, but diminishing as the magnetising force increases.

9. The increase of permanent magnetic permeability mentioned in 8 rises in greater proportion than the load up to a certain limit of the latter. Beyond this limit it rises in less proportion than the load, and eventually ceases to rise with increase of load; the value of the load at which this last occurs decreases as the magnetising force is increased.

10. For a wide range of loading, the effect of the stress on the permanent permeability is opposite in direction to the effect on the temporary permeability. Consequently loading may be found to produce either increase or decrease of permeability according as the permeability we are considering is temporary or permanent.

Similarly the total magnetic permeability, since it includes both temporary and permanent permeabilities, may be affected by loading in the contrary direction to the temporary magnetic permeability, and the more so as the effects of loading on the permanent magnetic permeability are very much larger than those on the temporary magnetic permeability for a rather wide range both of loading and of magnetising force.

11. The effect of loading—even when carried to a great extent—on the temporary permeability of unannealed piano-steel is very small, provided the wire be not permanently stretched by the load.

12. The effect of loading on the magnetic permeability of annealed iron varies very considerably with the amount of previous strain to which the metal has been subjected.

13. When the magnetising force is very considerable and the load small, permanent extension, resulting from previous loading, causes the diminution of temporary permeability produced by the load to be much increased; also the maximum diminution which can be temporarily produced by loading is increased.

When, however, the temporary load exceeds a certain limit, the diminution of temporary permeability produced by the load is lessened by permanent extension. Further, the load which produces maximum diminution of temporary permeability may be considerably lessened by permanent extension.

14. When the magnetising force is small the permanent strain may change *increase* of temporary magnetic permeability resulting from loading to *decrease*, provided the load does not exceed a certain limit.

When the above-mentioned limit is exceeded the effect of the permanent strain is reversed

15 The effects mentioned in 12, 13 and 14 are for the most part really the results of *subpermanent molecular strain*, and can be in great measure removed by severely shaking the wire

16 The permanent molecular strain which is left on the removal of any load, produces, both for low and high magnetising forces, a permanent diminution of magnetic permeability increasing with the strain up to a certain amount of the latter When, however, the strain is such that the wire is sensibly increased in length the *temporary* permeability *increases* considerably and the *permanent* permeability *diminishes* considerably up to a second limit of permanent strain when once more *decrease* of temporary permeability sets in

17 The first maximum of the decrease of permeability mentioned in 16 at first decreases with increase of the magnetising force to nearly zero it increases again however, if we exceed a certain limit of magnetising force

On the contrary the maximum increase of temporary permeability at first rises with the magnetising force until the permeability is more than doubled when it begins to fall as the magnetising force is pushed further

18 Mere rest after permanent extension has little or no effect on the alteration of the temporary permeability which is produced by loading whereas it very perceptibly increases the longitudinal elasticity of iron

19 For magnetising forces not exceeding a certain limit there are, for all temperatures between 0° C and 300 C two critical values of loading for which no alteration in the temporary permeability is produced by the load (see 1)

The value of the load at the first critical point diminishes, and that at the second critical point increases as the temperature is raised from 0° C to 100° C but as the temperature is raised still further the first critical load becomes greater and the second becomes less, until at some temperature between 250° C and 300 C the two critical points coincide

20 For magnetising forces exceeding a certain limit the two critical points of loading approach each other at first slowly and then rapidly, with increase of temperature from 0° C to 300° C Both critical loads diminish with rise of temperature, but the second more rapidly than the first

21 The effect of loading on the permanent permeability diminishes with rise of temperature from 0° C to 300° C

22 As the magnetising force increases the total magnetic permeability of annealed iron which has not been previously magnetised rises to a maximum and then begins to decline The maximum permeabi-

lity seems to occur at nearly the same point of magnetic *intensity* for different specimens of well annealed iron of good magnetic permeability, but not at the same point of *magnetising force*

23 When an iron wire has received previous magnetisation the point of maximum permeability occurs with a higher and higher magnetising force as the previous permanent magnetisation increases

The point of maximum permeability also occurs at a higher degree of magnetic *intensity* when the wire has been previously subjected to a high magnetising force

24 Besides a point of maximum total permeability there is a point of maximum *temporary* permeability which occurs a little before the first-mentioned point

24 and 23 are in accordance with Maxwell's extension of Weber's theory

25 The temporary permeability is diminished by previous permanent or subpermanent magnetisation in the same direction The effect above mentioned may be very considerable, provided the magnetising force lies between certain limits

26 When the wire is well shaken after having been previously magnetised by a strong force the temporary permeability is considerably restored, and is moreover much more nearly a constant for different values of the magnetising force than it was either previously to shaking or previously to suffering permanent magnetisation

27 More than 90 per cent of the whole magnetisation imparted by a given force to annealed iron may be permanent or subpermanent provided the magnetising force has a certain moderate value * When, however, the force is very large the percentage of permanent magnetism is much diminished

28 When an iron wire is loaded to a certain limit longitudinal magnetisation has no effect on the thermo electrical qualities of the metal

The limit of loading mentioned above seems to be the same for a given magnetising force as that at which magnetisation has no effect on the dimensions of the wire

29 The general features of the curves showing the relation between temporary magnetic permeability and load are the same for nickel as for iron

30 There are two critical points of loading at which the load has no effect on the temporary magnetic permeability of nickel

31 The load at the first critical point diminishes with diminution of the magnetising force and finally vanishes

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* This has been already noticed by Ewing ('Phil Trans 1885 Part II) in whose important memoir other points of interest connected with magnetic induction which are mentioned in this paper have been also discussed

* On the contrary the load at the second critical point increases as the magnetising force diminishes

32 The effect of increasing the magnetising force on both the first and second critical loads is therefore opposite in direction to the effect in the case of iron

33 Similarly the effects mentioned in 3 are opposite in direction in nickel and in iron

34 Rise of temperature from 0° C to 300° C increases the maximum increase of temporary magnetic permeability, which can be effected by loading nickel wire, and diminishes the maximum decrease

35 With nickel as with iron the magnetic permeability is not constant, but reaches a maximum The magnetising force which produces maximum permeability is greater with nickel than with iron but the magnetic intensity at the point of maximum permeability is less with nickel than with iron

36 Nickel wire can by shaking be more effectually de magnetised than iron

37 Well annealed nickel is capable of retaining subpermanently a very large percentage of the whole magnetisation imparted The maximum percentage retained is, however, not so great as with iron

38 At a certain temperature the magnetic permeability of nickel vanishes The temperature at which this occurs seems to be higher the higher the magnetising force This last, however may perhaps be due to impurities in the nickel

39 The magnetic permeability of nickel rises with the temperature to a maximum and then diminishes The temperature at which maximum permeability occurs diminishes as the magnetising force increases

40 The temporary effects of compression on the temporary magnetic permeabilities of iron, nickel, and cobalt are in the opposite direction to the effects of extension provided neither the mechanical nor the magnetic stress exceeds a certain amount

41 The temporary effect of traction transverse to the line of magnetisation on the magnetic permeability of iron, is opposite in direction to the effect of traction in the same line as the magnetisation

42 Temporary torsion beyond a certain limit (see 44) increases the temporary magnetic permeability of iron The amount of increase may become very large if the wire has previously suffered permanent torsion or permanent magnetisation in the opposite direction

43 Permanent torsion decreases the temporary magnetic permeability The amount of decrease may become very large if the

wire has previously received permanent torsion in the opposite direction

44 There is for all but very large magnetising forces a critical point of torsion, for which temporary torsion does not affect the temporary magnetic permeability

45 When the critical point of torsion is passed, the temporary permeability increases with the torsion at first more rapidly than the torsion and afterwards more slowly until a maximum is reached and the permeability begins to decline

46 When the wire has previously suffered excessive permanent torsion temporary torsion which has before produced increase of permeability now produces decrease

47 The effect of temporary torsion on the temporary permeability of unannealed piano steel wire is in the same direction as with annealed iron which has suffered excessive permanent torsion (see 46)

48 For a wide range of torsion the temporary permeability and the permanent permeability of annealed iron are oppositely affected by temporary torsion

49 Fluid pressure does not temporarily affect either the temporary magnetic permeability of annealed iron, or the permanent magnetisation of hard steel, except, it may be, to a degree which is not comparable with that of the effect of stress applied in any one direction

50 The application, however, or the removal of fluid stress like that of the stresses of compression, extension, and torsion, shakes out from annealed iron a certain amount of residual magnetism

IV. "Note on a New Constituent of Blood Serum" By L C Wooldridge, M D, D Sc, Research Scholar to the Grocers' Company Communicated by Dr Pye-Smith, F R S Received March 19, 1887

I wish in the present note to draw attention to a proteid substance which exists in very small quantity in blood serum. Owing to the difficulty of obtaining a sufficient amount, I shall not attempt to give a complete description of its chemical characters, but shall confine myself chiefly to its physiological properties which, I venture to suggest, possess considerable interest It is obtained by rendering undiluted serum distinctly acid by means of dilute acetic or very dilute (4 pro mille) sulphuric acid Neutralisation does not cause its precipitation, the serum must have a strong acid reaction It is constantly present in the serum of dog's blood, and when collected by the centrifuge it is precisely similar in physical characters to ordinary fibrin, and only differs from the latter chemically by being more easily soluble in dilute alkali. It is totally different from the soft granular precipitate of paraglobulin, the latter substance being extremely easily soluble in the slightest excess of acid. It is also constantly present in serum of sheep's blood. In the case both of dog's blood and sheep's blood it is only present in very small amount, and in the serum from horse blood and bullock's blood it was absent in the specimens I have examined. The physiological interest of this substance will be seen from the following.

It is well known that Schmidt regarded two proteid substances as being essential for coagulation. One of these bodies was paraglobulin, a substance existing in large quantity in blood serum. Subsequent investigation has failed to confirm this view, and there can be no doubt that paraglobulin is not essential to the process. But Schmidt has obtained results, the correctness of which we are in no way entitled to dispute, which apparently clearly show that the quantity of fibrin formed can be largely increased by the addition of paraglobulin. I think this discrepancy can be explained by the help of this new substance, and this will be best shown by describing the following experiments.

Two portions of peptone plasma were taken, and

To No. 1, an equal quantity of sheep's serum was added.
,, No. 2, a small quantity of a solution of the new substance.

No. 1, after many hours only presented a scarcely perceptible flocculus of fibrin.

No 2 was quite solid in 15 minutes; on squeezing out the fluid from the clot and again adding a solution of the new substance, the mixture again clotted through and through.

Now Schmidt's experiments were very much of this nature. He found in certain specimens of hydrocele fluid that the addition of fibrin ferment produced very slight clotting, whereas on the further addition of a substance which he regarded as paraglobulin a decided clotting took place. Now sheep's serum contains plenty of paraglobulin and plenty of fibrin ferment, but it has no appreciable effect in my experiments.

But this new substance, which it must be remembered is only present in very small quantity in serum, had the most marked influence, and hence I conclude that it is the new substance, and not paraglobulin, which increases the amount of fibrin. It may be mentioned that in preparing paraglobulin a certain amount of the new substance is always precipitated with the former substance.

A second physiological property of this new substance is the effect it exerts when injected into the circulation of a living animal.

It is very exceptional to find that the injection of blood serum produces any effect, serum containing plenty of paraglobulin and ferment but only traces of the new substance

But the injection of a solution of this body prevents the coagulation of the shed blood Occasionally as the result of the injection very small thrombi are formed possibly if more could be obtained considerable intravascular clotting might be set up

The following is an example

A quantity of the new substance obtained from 300 c c sheep's serum and well washed was dissolved in dilute alkali and salt solution (The amount of substance was I estimate 0 2 gram) This solution was injected into the jugular vein of a rabbit The blood of this rabbit previous to the injection clotted in two minutes after the injection the blood drawn off remained quite fluid for three hours—time of observation It clotted, however, directly on adding some of the solution injected

The injection of considerable quantities of serum or of paraglobulin I have not found to have any appreciable effect

Of itself this substance, since it exists in so small amount, is of little interest, but as it appears to vary in quantity in different animals and under different circumstances, it is easy to see that misapprehensions as to the influence of paraglobulin on coagulation might easily arise

These observations also throw great doubt on the power of fibrin ferment to produce a so called intoxication

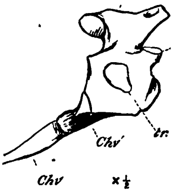
This substance has an extremely feeble influence on dilute $MgSO_4$ plasma, and hence contains but a trace of fibrin ferment Since it is closely related to the fibrin yielding matters of the plasma, and to the tissue fibrinogens I have elsewhere described, I should propose to call it serum fibrinogen

V. "Preliminary Note on the Fossil Remains of a Chelonian Reptile, *Ceratochelys sthenurus*, from Lord Howe's Island, Australia" By THOMAS H HUXLEY, F R S Received March 24, 1887.

The interesting remains of which I propose to give a brief notice in the present communication, are contained in a friable sandstone (apparently formed of concreted blown sand), and they have a very recent appearance The age of the deposit in which they are found is unknown, but it is probably quaternary The specimens have been for some years in the palæontological collection of the British Museum, and, for the most part, they have not yet been submitted to careful examination. But I learn that the greater number of them were long since rightly determined to be Chelonian by Mr. Davis, and set aside as such.

Several of the most important of these numerous and, in general, very fragmentary bones were originally found imbedded close together in the same block of sandstone. They consist of a great part of a pelvis, a caudal vertebra, and an imperfect skull. Of the pelvis, a right ischium and a pubis are imbedded in the rock, while an imperfect right ilium, which fits well on to the ischium, is separate; all these bones are unmistakably Chelonian. The caudal vertebra has remarkable peculiarities. It resembles an ordinary Chelonian caudal vertebra from the anterior half of the tail, in its general characters; but it is strongly opisthocœlous, the centrum having a deep cup behind and a correspondingly curved articular head in front. From the posterior part of the ventral face, two stout processes diverge, and present terminal rounded facets for the rami of the large chevron bone which must have articulated with them. As a general rule, the caudal vertebræ of Chelonia are procœlous—but *Chelydra* and *Gypochelys* (perhaps also *Staurotypus* and *Platysternum*) form well known exceptions,[*] in so far as the vertebræ behind the 3rd or 4th are strongly opisthocœlous. In fact, the vertebra in question closely resembles the 6th or 7th of *Chelydra* or of *Gypochelys* (see figs. 1 and 2). In the first, however, the transverse processes are

FIG. 1.



Caudal vertebra of *Ceratochelys.* *N*, platform on the neural arch; *pz*, prezygapophysis mutilated; *tr*, broken transverse process; *Chv′*, processes for the chevron bone; *Chv*, chevron bone.

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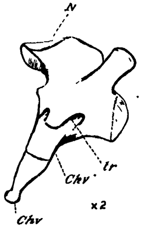
[*] The opisthocœlous character of most of the caudal vertebræ of *Chelydra* was first pointed out by Von Meyer in his description of the Œningen *Chelydra*. Baur ("Osteologische Notizen," 'Zool. Anzeiger,' No. 238, 1886) has gone fully into the

FIG. 2.



Caudal vertebra of *Chelydra*. Letters as in fig 1.

very much stronger and the pentagonal platform into which the upper surface of the neural arch expands, in place of a neural spine, is as long as the vertebra instead of being only about half as long. The stout pre-zygapophysis of the right side is broken off, leaving only the base visible in the fossil.

Two other caudal vertebræ, having the same structural features, occur among the detached remains; and belong, like the first, to the second fourth of the tail. Another tolerably complete vertebra, with a considerably longer centrum, corresponds very closely with a caudal vertebra of *Gypochelys* from the third fourth of the tail. In this, as in one of the foregoing vertebræ, the chevron bones are ankylosed with the centrum. I conceive, then, that there can be no doubt that the pelvic bones and these caudal vertebræ belonged to a Chelydroid Chelonian, of about the size of the largest "Snapping turtles" which are met with in North America at the present day.

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question, and has pointed out the exceptional nature of their structure among the Chelonia. Since the above paragraph was written, Dr. Günther has kindly enabled me to examine a spirit specimen and a skeleton of *Platysternum*. The caudal vertebræ resemble those of *Chelydra*, except that the last nine are procœlous, while that between these and the more anterior opisthocœlous vertebræ is nearly flat at the ends. In this, as in other respects, *Platysternum* presents characters intermediate between *Chelydra* and the ordinary *Emydæ*. Professor Cope ('Vertebrata of the Tertiary Formations of the West,' 1883, p. 111) ascribes opisthocœlous caudal vertebræ to the *Baenidæ*, but no figures or descriptions of such vertebræ are given. Of the opisthocœlous Chelonian vertebræ figured in Plate XXIV of the 'Report of Extinct Vertebrata obtained in New Mexico' (1877) it is expressly stated that their "correct reference cannot now be made" (p. 43).

Prima facie, the skull found in the same block might also be expected to be that of a Chelydroid and, in fact, it is so I do not base this interpretation on the Chelonian character of the upper jaw as there are various extinct Saurian reptiles which closely approximate Chelonia in this part of their structure The diagnostic characters lie in the back part of the skull and especially in the auditory region, which is altogether Chelonian Not only so but when this fragmentary skull is compared with that of *Chelydra* the correspondence between the two is singularly exact (figs 3 and 4) In two respects however, the fossil differs from *Chelydra* and *Gypochelys*

FIG 3

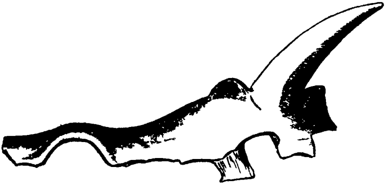
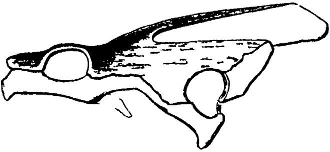


FIG 4.



Skulls of *Ceratochelys* (fig 3) and *Chelydra* (fig 4) the latter of the nature size the former much reduced The portion of the skull of *Chelydra* which corresponds with the fossil is shaded

1 The roof over the temporal fossa formed by the parietal, post-frontal, and other bones, which leaves the auditory region uncovered in the recent genera,* extends back, beyond the occiput, in the fossil, and sends down a broad vertical rim from its margin

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* The roof extends much further back in *Platysternum*

2. The upper surface of the cranial shield is, at most, rugose in the recent *Chelydridæ;* in the fossil, three strong conical processes, like horn-cores, of which the middle is the longest, are developed from its posterior and lateral region *

This skull is described and figured in the 'Philosophical Transactions' for 1886 (Plate 30, fig 1) by Sir R Owen, under the generic or subgeneric name of *Meiolania,* and is said to belong to a Saurian reptile closely allied to the "*Megalania prisca*" described in earlier communications But the skull is assuredly that of the Chelydroid Chelonian to which the pelvis and caudal vertebra belong What *Megalania prisca* may be I do not pretend to say, but the remains which I have described can have nothing to do with any Saurian reptiles, and I propose to confer on the genus of Chelonia to which they belong the name of *Ceratochelys*

The singular osseous caudal sheaths described by Sir R Owen, in the same memoir, also appertain to *Ceratochelys* They formed part of the series of remains sent to the British Museum along with the foregoing, in which none but Chelonian bones have yet been discovered, and the remains of vertebræ left in these sheaths are similar to the caudal vertebræ of the terminal fourth of the tail in the *Chelydridæ* The Snapping turtles are noted for the length and strength of the tail and for the strong, laterally compressed, acuminated "scales" which form a crest along the median dorsal line, while others, less strongly keeled, lie at the sides of the tail In many Chelonia, the extremity of the tail is enveloped in a continuous sheath These and other scale-like structures in the Chelonia, are usually spoken of as if they were entirely epidermal But, a day or two ago, Dr Gunther informed me that in the Australian Tortoise, *Manouria,* the great imbricated scales of the limbs contain bony scutes, and that similar scutes are to be found in *Testudo græca* This of course, suggested the examination of the caudal scales of *Chelydra* and *Gypochelys,* and, having been enabled by Dr Gunther's kindness to examine the caudal scales of a good sized specimen of the latter, I have found that those of the crest contain bony scutes † The bony scute corresponds very closely in form with the whole "scale," but the recurved apex of the latter is formed only by epidermal substance (figs 5 and 6)

The living *Chelydra,* therefore, has a caudal armature which, in

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* It is possible that these may be dermal bones coherent with the proper cranial shield

† The fact is noted by Rütimeyer (Lang and Rutimeyer, "Die Fossilen Schildkröten von Solothurn," 'Denkschriften der Allg Schweiz Gesellschaft,' vol. 22). The armature of the tail in *Platysternum* is for the most part arranged in zones, of four plates in each zone, but I have not yet been able to find any bone in them.

FIG. 5.

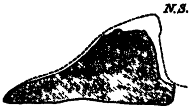
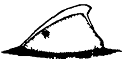


FIG. 6.



Sectional views of a scute of the tail-armour of *Ceratochelys* (fig. 5), and of one of the crest plates of *Gypochelys*, both of the natural size.

principle, is similar to that of *Ceratochelys*, but the osseous elements are relatively atrophied. There is exactly the same relation between the armour of species of living *Crocodiles* and *Alligators*, on the one hand, and those of *Jacare* and *Caiman* and the extinct *Teleosauria*, on the other. In the former, the epidermal scales remain well developed on the ventral side of the body, while the corresponding osseous scutes, fully developed in *Jacare*, *Caiman*, and *Teleosauria*, have vanished.

Among the detached fragments to which I have referred, there are remains of ribs, with their costal plates; marginal and other plates of the carapace; parts of the plastron; part of a scapula; sundry limb bones; and several of the cranial processes called "horn-cores." They all agree, so far as they can be compared, with the determination already arrived at; which, to sum it up in a few words, is that the remains of crania and caudal sheaths from Australia, hitherto referred to Saurian reptiles, under the names of *Megalania* and *Meiolania*, appertain to a hitherto unknown species of Chelonian, *Ceratochelys sthenurus*, closely allied to the living *Chelydra*, *Gypochelys*, and *Platysternum*.

The evidence of this fact offered in the present note appears to me to be conclusive, but it may be desirable hereafter to figure the parts mentioned and to describe them at length.

The interest which attaches to the discovery of this singular Chelonian arises partly from the fact, that the group of Chelonia to which it belongs is wholly unrepresented in the fauna of Australia, as at present known. *Platysternum* is usually said to be found in China. Dr. Günther, however, informs me that Upper Burmah is its proper habitat; otherwise, North America, east of the Rocky Mountains, is the nearest region in which the *Chelydridæ* are to be found. But *Chelydridæ*, and, indeed, species of the genus *Chelydra*, occur in Upper Miocene (Œningen) and in Eocene formations in Europe. Moreover, *Platychelys*, of the Upper Jurassic series of Bavaria and Switzerland is regarded by Rütimeyer as an early form of the group.

Lord Howe's Island is about 200 miles from the nearest Australian mainland, and something like 400 miles, as the crow flies, from the Darling Downs, in which the caudal armour, which has been ascribed to *Megalania*, was found. The discovery of *Ceratochelys*, therefore, has an interesting bearing on the question of the former extension of Australia to the eastward, on the one hand; and of the possible derivation of such forms as *Ceratochelys* from Asia, on the other hand. An elevation of the sea bottom of 6000 feet would place Norfolk Island and Lord Howe's Island on a peninsula extending from the region of the present Barrier Reef to New Zealand; and the Floræ and Faunæ of those islands are known to have special affinities with those of New Zealand and none with those of Australia.

Speculations respecting the origin of the Chelonian carapace, are suggested by the discovery of osseous scutes in the vertebral region of the tail, and their coalescence in *Ceratochelys* to form a sort of caudal carapace, ridged in a manner resembling that of *Chelydra* and *Platychelys*. But the consideration of these points would take me beyond the limits of the present note.

VI. "Action of Caffein and Theine upon Voluntary Muscle." By T. LAUDER BRUNTON, M.D., F.R.S., and J. THEODORE CASH, M.D. Received March 24, 1887.

From a number of experiments we have found that caffein and theine both cause rigor in the voluntary muscles of frogs. All these experiments were made on *Rana temporaria* and none on *Rana esculenta*. The action is, however, very variable, the rigor being sometimes exceedingly well marked, and at other times not observable. The alteration does not depend on the dose of the alkaloid. When the gastrocnemii of the same frog were treated with solutions of caffein or theine of different strengths, the stronger solution had the most powerful action; but when different frogs were used, a large dose sometimes had little action on one frog, while a small dose had a powerful action on another. Theine seems to be rather more powerful than caffein, but the quantitative difference between them is slight. There is, however, a marked qualitative difference between them, inasmuch as theine tends to produce rhythmical contractions in the muscle. Complete curarisation quickens the occurrence of rigor. A variation is observed in the action of the alkaloids on the different muscles of the same frog. In the triceps the rigor is more rapidly developed, and more extensive than in the gastrocnemius. In the sartorius rigor commences soon, increases rapidly, lasts for some hours, and then relaxes.

The addition of lactic acid to a solution of theine or caffein causes the rigor to appear sooner, develop more rapidly, and attain a greater maximum. Potash retards and diminishes the action of theine or caffeine. Guanidine produces at first its characteristic clonic contractions, but these pass off long before the rigor of caffeine or theine begins, and the appearance of the rigor is postponed as compared with the rigor of theine or caffein alone. On comparing the effect of guanidine with that of chloride of barium, we found that in the case of the guanidine the rigor was longer in occurring, and its maximum was greater than that produced by barium salts. The addition of chloride of calcium to the solution of theine quickens rigor and makes it more extensive. One phenomenon which seems deserving of attention is the rhythmic contraction of the muscle produced by theine. This rhythm is so slow that it would escape attention unless a very low rate of speed were used in the recording apparatus; it is sometimes as slow as from three to about one contraction per hour; it may continue for twenty hours. The rhythm is usually produced by small doses of theine, which do not cause a marked rigor; it may, however, occur at the commencement of what develops into a lasting rigor, or at the relaxation of a pseudo-rigor, by which we mean a phenomenon which might also be termed tetanic relaxation. The rhythm is more rapid at the commencement of its occurrence and slower towards its termination; it may be as rapid as twenty relaxations and contractions in an hour, or as slow as between one and two in an hour. The total extent of contraction and relaxation is very small, amounting to about one-fifth of a millimetre At first the contractions and relaxations are equal in duration, but afterwards the relaxations become more rapid and the contractions slower. In one instance we observed the remarkable phenomenon to which we have given the name of pseudo-rigor; in this experiment the application of the theine was followed by slight relaxation of the muscle, to this succeeded an equal contraction, and then followed great relaxation below the normal, so great indeed that the negative curve below the abscissa strongly resembled the positive curve of contraction due to rigor in most other experiments.*

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* This phenomenon is difficult to explain, but it suggests the possibility of a transverse as well as a longitudinal contraction in muscular fibre.—March 29, 1887.

VII. "Contributions to our Knowledge of the Connexion between Chemical Constitution and Physiological Action. Preliminary Communication on the Action of certain Aromatic Bodies." By T. LAUDER BRUNTON, M.D., F.R.S., and J. THEODORE CASH, M.D. Received March 24, 1887.

The distinctive action of the lower members of the fatty series is their stimulant and anæsthetic action on the nerve-centres.

The members of the aromatic series also affect the nervous system, but they appear to affect the motor centres more than the sensory, so that instead of producing anæsthesia, like the members of the fatty series, they tend rather to produce tremor, convulsions, and paralysis. Benzene, chlorobenzene, bromobenzene, and iodobenzene are all somewhat similar in their action on frogs; the halogen radicals not modifying the action of the benzene to such an extent as they do in the case of ammonium salts. The voluntary muscles are weakened by them, and there is a slight tendency to paralysis of the motor nerves; but the action is chiefly exerted upon the brain and spinal cord. The brain is first affected, as shown by general lethargy and disinclination to move. Next the cord is affected; motions are imperfectly performed, and there is a tendency to general tremor on movement resembling that observed in disseminated sclerosis; sometimes, however, the tremor is observed independently of movement.

The addition of hydroxyl to the benzene nucleus intensifies the convulsant action, so that oxybenzene (carbolic acid) and dioxybenzene cause convulsions in frogs, and trioxybenzene causes jerkings, though of a slighter character.

The Society then adjourned over the Easter Recess to Thursday, April 21st.

Presents, March 31, 1887.

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Ter Gouw (J) Geschiedenis van Amsterdam Deel V 8vo *Amsterdam* 1886 Map of Amsterdam in 1544 Twelve sheets *Reprint* Obl 4to *Amsterdam* 1885
The Magistracy of Amsterdam.

"On the Effect of Polish on the Reflexion of Light from the Surface of Iceland Spar." By C. SPURGE, B.A., St. Catherine's College, Cambridge. Communicated by R. T. GLAZEBROOK, M.A., F.R.S.

Received November 18,—Read December 16, 1886. Revised March 3, 1887.

I *Introduction.*

The optical effect of polishing the surface of a transparent body has received the most complete investigation at the hands of Seebeck,[*] and till very recently[†] Seebeck's were almost the only experiments made on the subject. Seebeck's method consisted in observing with a Nicol the light of a lamp reflected from the surface of the body. By means of a divided circle, the angle of polarisation was measured, and it was from an alteration in this angle that a change in the state of the surface was inferred. But it has been since shown by Jamin[‡] that, when *plane* polarised light is incident upon the surface of a transparent body, the reflected light is in general not plane but to a measureable degree elliptically polarised, and consequently there is no angle of incidence at which the light can be *completely* quenched by a Nicol. It follows that, as regards our present state of knowledge, Seebeck's investigation is to some extent incomplete, and also that there is some uncertainty in the determination of the angles of polarisation, which may affect our conclusions as regards the state of the surface, especially since the difference produced by polishing is according to Seebeck not very large. Both Sir David Brewster[§] and M. Jamin were of the opinion that Seebeck's experiments should be repeated, and the latter promised to consider the effect of polish later on but appears never to have done so. Mr. Glazebrook kindly pointed out to me that the subject presented a suitable field for research, and, at his instance, I undertook the present investigation.

My object has been to attain *greater accuracy* than hitherto by employing for an analyser a quarter undulation plate in addition to a Nicol, so as to make the extinction of the reflected light very complete. The angle of incidence of the polarised light falling on the surface of the crystal was kept constant, in order to measure as *directly* as possible the alteration produced by change of polish Both the azimuth of the major axis and the ratio of the axes of the elliptically polarised light were calculated. These quantities furnish us with *two* independent tests of a change of surface, and completely determine the nature of the light, so that a knowledge of their values before and after polishing enables us to state the precise alteration produced in the reflected light, a question which has never been investigated, and is, I believe new to this paper.

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[*] 'Poggendorff, Annalen,' vol. 20, 1830, p 27 . vol. 21, 1831, p 290.

[†] Sir J. Conroy, 'Roy Soc. Proc,' Feb , 1886

[‡] 'Annales de Chimie,' vol. 29, 1850, p. 263.

[§] 'Edinb. Journ. Sci.,' vol. 6, 1831.

II. *Apparatus.*

A series of preliminary experiments were made to discover what apparatus was best suited for the investigation. I found that, whether a Nicol or a Nicol and a quarter undulation plate were used as analyser, it was best to polarise the light before incidence. Also, observations showed me that a Nicol and a quarter wave plate were a more sensitive arrangement than a simple Nicol, supposing that in each case the light was polarised before incidence. I have therefore deemed it necessary to employ the Nicol and quarter wave plate arrangement in order to secure all the accuracy that is possible by completely quenching the reflected light.

The instrument I used was an elliptic analyser kindly lent for the purpose of these experiments by Professor Stokes

A very full description of the instrument will be found in the 'Phil. Mag' for 1851, but the following abbreviated account is given in order to explain the way in which it was used during the course of the experiments.

The elliptic analyser consists of a brass annulus attached to a vertical stem which fits into a hollow cylindrical foot. When the foot is placed on a table, the plane of the annulus is vertical. Within the annulus turns a brass graduated disk; and the angle through which it turns is read off by means of verniers engraved on the annulus. These verniers are therefore fixed. The disk is pierced by a central aperture on the side of which opposite the incident light is a screw thread, so that a cell containing a quarter wave plate can be screwed into the disk. In front the disk carries a hollow cylinder turned in the lathe with the disk itself. Round the cylinder turns a collar into which is screwed a tube containing the analysing Nicol. The collar carries a pair of level edged verniers by which the angle may be read off through which the Nicol has been turned. These verniers are therefore moveable. Thus the quarter wave plate moves in azimuth, carrying the Nicol along with it, and the Nicol has likewise an independent motion in azimuth. In observing, the light is extinguished by a combination of the two movements, in which case the elliptically polarised light is converted by the quarter wave plate into plane polarised, which is then quenched by the Nicol. There are two principal positions in which the light can be quenched, and, since either Nicol or quarter plate may be reversed by turning through 180°, there are four subordinate positions corresponding to each principal position. The position of the Nicol is determined by the readings of the two moveable and that of the quarter wave plate by the readings of the two fixed verniers. Thus each principal position is determined by eight readings, and in the tables which follow, each number is the mean of eight readings.

Suppose that R, R′ are the mean readings of the fixed, r, r' the mean readings of the moveable verniers, then the quantities, which it is the object of the present investigation to determine, are $\tan \varpi$, the ratio of the axes of the ellipse, I, the azimuth of the major axis of the ellipse, and these are given by the formulæ—

$$\cos 2\varpi = \sin (r'-r)/\sin (\mathrm{R}'-\mathrm{R}),$$

and

$$\mathrm{I} = \tfrac{1}{2}(\mathrm{R}'+\mathrm{R}).$$

These equations determine ϖ absolutely, but I will be measured from an arbitrary zero which will remain fixed so long as the quarter plate is not unscrewed from its containing tube, but which will be changed by a constant amount, if for any reason it is unscrewed and rescrewed up.

A subsidiary quantity is ρ, the retardation of the crystal plate, which may be determined by means of the equation,

$$\cos \rho = \tan (r'-r)/\tan (\mathrm{R}'-\mathrm{R}).$$

The source of light employed was an Argand burner, the rays from which were polarised by means of a Nicol before incidence.

III. *Adjustments.*

The tube of the polariser was levelled and its axis placed in a direct line with the centre of the flame. The height of a small brass table, on which the crystal was placed was adjusted so that the reflected light passed through the tube of the analyser. A number of preliminary observations were made to determine the best angle of incidence, *i.e.*, the angle at which the extinction was most rapid.

The best position of the analyser having been found, the centre of the tube of the analyser was adjusted to the same height as the centre of the tube of the polariser and the centre of the face of the crystal. The tube of the analyser was so directed that a ray of light from the centre of the flame passing along the axis of the polariser was reflected so as to enter at the centre of the tube of the analyser and leave at the centre of the tube.

As the present experiments were directed to discover a difference which at the outset was recognised as possibly small, especial care was taken to secure fixity of position in the parts of the instrument and in the position of the face of the crystal of Iceland spar.

The instruments were firmly attached to a laboratory table, and before commencing the moveable parts were examined and tightly screwed up. Round the base of the table and the foot of the analyser a small quantity of melted paraffin was poured so as to form connecting links from one part of the apparatus to another. From time to time these links were examined and found to be unbroken.

To attach the crystal to the table, hard electrical cement was used. The plan finally adopted was to place the crystal on the table, and to fix it by pouring a small quantity of melted wax down the back of it.

Since the crystal was removed from the table to polish its surface, some means of restoring it to its original position were needed. The following optical method was employed.

A circular diaphragm with a central pinhole was fitted to the brass tube containing the polarising Nicol. If the pinhole were slightly eccentric, the position of the hole would change as the disk rotated in its plane. To obviate any such alteration, a radius was drawn on the diaphragm which was set so that it was horizontal and always pointed in the same direction. In front of the source of light was placed a screen having a small hole at the same height as the pinhole in the diaphragm. The position of the screen was defined by lines drawn on the table.

Thus only a single ray of light was allowed to fall on the surface of the crystal, viz., that passing through the apertures in the screen and diaphragm. As these apertures could always be replaced in the same position, the direction of this incident ray was a fixed horizontal straight line. In a similar manner a circular diaphragm with a central pinhole was fitted to the brass tube containing the analysing Nicol, and some distance in front of this tube was placed a screen with a pinhole at the same height as the pinhole in the diaphragm. The position of the screen was defined as before by lines drawn on the table. A radius was drawn on the diaphragm, and also, since the tube itself was moveable, a mark was made on it so that it could be turned into the same position.

The crystal was placed on the brass table so that its plane was vertical and passed through the centre of the circular top.

On placing the eye opposite the aperture in the screen facing the elliptic analyser, it was found that a bright dot of light was visible. Thus the horizontal incident ray already mentioned must have been reflected by the crystal surface so that it passed through the apertures in the screen and the diaphragm fitted to the analysing tube. These apertures could be replaced in the same position, and therefore the direction of the reflected ray was a fixed horizontal line. Consequently the normal to the surface of the crystal bisecting the angle between the incident and reflected rays was a fixed direction.

Supposing the crystal to have been taken down for polishing, it could be restored to position by placing the diaphragms and screens in their proper stations, and setting the crystal so that a bright dot of light was visible to the eye in front of the last screen.

The only possible changes in position that this method allows are a displacement parallel to the table and a rotation of the face of the crystal in its own plane. The former was prevented by means of fixed marks on the table, and the latter by taking every precaution to leave the base of the crystal in contact with the table unchanged, and later on by the use of a template.

A series of experiments were made to determine whether the diaphragms and screens could be removed and replaced in exactly the same positions For this purpose the screens and diaphragms were removed one at a time and replaced, using only the setting lines. It was found that the dot of light remained visible, while the slightest displacement of the screens caused it to disappear.

During each set of experiments the screens and diaphragms were frequently replaced to determine if the crystal remained unmoved.

The surface of the crystal was shielded during the day by a box with apertures, and completely covered at night. In taking the readings the quarter wave plate and Nicol were turned into such positions that the centre of the field was as dark as possible.

IV. *Experiments made with a Natural Face.*

Observations were now made with the light reflected from that natural face of the crystal which seemed the best. The results are given in Tables I, II. The observations made with the crystal were always consecutive, none being rejected after the first satisfactory observation had been taken.

Table I.—Observations with a Natural Face. Mean Temp. 15°·3 C.

	r'.	r.	R′.	R.	$(r'-r)$.	(R′−R).
	90·562°	4·601°	153·895°	62·475°	85·961°	91·420°
	90·585	4·565	153·781	62·286	86·020	91·495
	90·350	4·334	153·955	62·256	86·016	91·699
	90·293	4·324	154·002	62·516	85·969	91·486
	90·285	4·525	153·840	62·462	85·760	91·378
	90·454	4·594	153·852	62·314	85·860	91·538
	90·278	4·430	153·790	62·408	85·848	91·382
	90·362	4·321	153·888	62·315	86·041	91·573
	90·486	4·470	153·866	62·339	86·016	91·527
	90·045	4·525	153·833	62·215	85·520	91·618
	90·287	4·545	153·433	62·317	85·742	91·116
	90·370	4·470	154·020	62·264	85·900	91·756
	90·312	4·450	153·687	62·285	85·862	91·402
Means..	90·359	4·473	153·834	62·342	85·886	91·492

Table II.

	ϖ.	tan ϖ.	I.
	1° 53′·5	0·03303	108·185°
	1 50·8	0·03225	108·033
	1 48·1	0·03145	108·106
	1 52·5	0·03275	108·259
	2 0·3	0·03501	108·151
	1 55·3	0·03356	108·083
	1 57·5	0·03419	108·099
	1 49·	0·03172	108·101
	1 50·4	0·03212	108·102
	2 5·4	0·03648	108·024
	2 3·2	0·03585	107·875
	1 51·2	0·03236	108·142
	1 56·8	0·03399	107·986
Means..	1 54·9	0·03344	108·088

Taking from Table I the mean values of R′−R, r′−r, we have—

$$\cos 2\varpi = \frac{\sin(r'-r)}{\sin(R'-R)} = \frac{\sin 85\cdot886}{\sin 91\cdot492} = 0\cdot997762.$$

Thus $\varpi = 1^\circ 55'$, and we obtain for the values of the quantities which determine the nature of the polarised light,

$$\tan\varpi = 0\cdot03346, \qquad I = 108\cdot088^\circ.$$

We have to find whether these two quantities are altered by polishing.

The subsidiary quantity ρ is given by—

$$\cos\rho = \frac{\tan(r'-r)}{\tan(R'-R)} = \frac{\tan 85\cdot886}{\tan 91\cdot492} = -0\cdot3621.$$

We are calculating ρ merely for the purpose of verification, and are not using it to determine the nature of the polarised light. Take, then, ρ to be the least positive angle which satisfies the last equation. Thus $\rho = 111^\circ 14'$. This is the mean value of ρ. To estimate the error in determining ρ, take the 12th set of observations in Table I. We have—

$$\cos\rho = \frac{\tan 85\cdot900}{\tan 91\cdot756} = -0\cdot42768,$$

whence $\rho = 115^\circ 19'$. There is thus a difference of 4° in the extreme value of ρ from the mean. The cause of this apparently large variation in the values of ρ will be considered later on.

V. *Determination of the Inclination of the Natural Face to the same Face Polished.*

The crystal was taken down from its position on the brass table, and, in order to ensure that the face from which the light was reflected was ground parallel to itself, the inclinations of the face to two other faces which were left untouched, were obtained before and after polishing. The measurements were made in the usual way with the spectrometer. It was found that a fair image of the slit could be obtained by reflexion at each of the faces. Three times the crystal was completely dismounted and measurements of each angle taken.

	Before polishing.		After polishing.	
	First angle.	Second angle	First angle.	Second angle.
	105° 5′ 56″	74° 58′ 37″	105° 6′ 37″	74° 59′ 0″
	105 6 15	74 59 0	105 5 7	74 58 22
	105 7 15	74 58 7	105 5 7	74 58 50
Means..	105 6 29	74 58 35	105 5 37	74 58 44

The angles are almost unaltered and the differences are within the limits of experimental error. Lines were drawn round the sides of the crystal parallel to the edges, and after polishing remained still parallel, which was an additional confirmation. The conclusion at which we arrive is that the polished face was parallel to the natural face.

VI. *Method of Polishing the Crystal.*

The natural face which had been the subject of the experiments recorded in Tables I and II was polished. The polishing was performed by myself to ensure an exact knowledge of the treatment it received.

I am indebted to Professor Threlfall for the use of the apparatus and materials. As previous experimenters seem to have experienced a difficulty in obtaining a surface polished in the same manner, it may be well to state the exact mode of polishing the surface.

That the crystal might be polished under the same conditions of pressure, a rectangular block of lead was cemented to the crystal. In polishing especial care was taken not to press on the crystal downwards, but to exercise only lateral pressure. The crystal was first polished with emery on a plate of glass which had been rendered plane by grinding on a slate. The emery was prepared as follows:

some very fine emery was scattered over a tub of water and allowed to settle; after standing for a number of minutes, the liquid was poured off, and the sediment, which was deposited on standing for a further number of minutes, was preserved for use. Five kinds of emery were used, viz.:—

Stood	1 minute,	deposited in	5 minutes.
"	5	"	15 "
"	15	"	30 "
"	30	"	2½ hours.
"	2 hours	"	40 "

The crystal was polished about twenty minutes with each kind in a perfectly quiet room to avoid dust being deposited on the glass and causing flecks in the crystal.

Next a bed of refined pitch was prepared having its upper surface perfectly plane. Upon this the crystal was polished with rouge for about three hours. Finally, the surface was carefully cleaned by washing it in a stream of water.

VII. *Experiments made with the same Face Polished.*

The screens were carefully set in their places, and by means of these the crystal was fixed in the same position as formerly by the method described towards the end of Section III. Then the observations recorded in Table III were taken consecutively.

Table III.—Observations with a Polished Face.
Mean Temp. 14·85° C.

	r'.	r.	R′.	R.	$(r'-r)$.	(R′−R).
	91·195°	4·612°	153·714°	61·798°	86·583°	91·916°
	91·089	4·402	153·612	61·936	86·687	91·676
	90·866	4·385	153·545	61·954	86·481	91·591
	91·048	4·325	153·781	62·163	86·723	91·618
	91·162	4·194	153·229	62·095	86·968	91·134
	90·871	4·400	153·406	61·969	86·471	91·437
	91·008	4·189	153·376	62·095	86·819	91·281
	90·884	4·166	153·637	62·110	86·718	91·527
	91·020	4·191	153·465	62·244	86·829	91·221
	91·244	4·061	153·620	62·287	87·183	91·333
	91·066	4·118	153·736	62·295	86·948	91·441
	90·907	4·084	153·410	62·200	86·823	91·210
Means..	91·080	4·260	153·544	62·095	86·769	91·449

Table IV.

	ϖ.	tan ϖ.	I.
	1° 24′ 9	0 02470	107 756°
	1 25 7	0·02498	107 ·774
	1 34 ·2	0 02741	107 749
	1 25 5	0 02488	107 972
	1 24 4	0 02455	107 662
	1 36 8	0 02816	107 ·687
	1 27 4	0 02548	107 785
	1 27 2	0 02537	107 878
	1 27 8	0 02555	107 855
	1 14 5	0·02168	107 ·968
	1 20 8	0 02349	108 015
	1 28 1	0 02565	107 ·805
Means..	1 26 6	0 02515	107 ·819

Taking from Table III the mean values of $R'-R$, $r'-r$, we have—

$$\cos 2\varpi = \frac{\sin 86{\cdot}769}{\sin 91{\cdot}449} = 0{\cdot}99873.$$

Thus $\varpi = 1°\ 26\ 6'$, and we obtain for the values of the quantities, which we are seeking in order to determine the nature of the polarised light,

$$\tan \varpi = 0{\cdot}02520, \qquad I = 107{\cdot}819°.$$

Comparing these values with those formerly obtained which follow Table II, we see that the effect of polishing is to cause a small alteration of the ratio of the axes and in the inclination of the major axis of the ellipse. Thus the ratio of the axes has been changed from 0·03346 to 0 02520, while the inclination has been changed from 108 088° to 107·819°, an alteration of about 16′. These results also show that the reflected light is exceedingly nearly plane polarised.

Again, the Tables III give for the subsidiary quantity ρ,

$$\cos \rho = \frac{\tan 86\ 769}{\tan 91{\cdot}449} = -0{\cdot}44808,$$

so that $$\rho = 116°\ 37'.$$

The value of ρ obtained before polishing was—

$$\rho = 111°\ 14'.$$

These values are not the same, but this has no bearing on the polishing, inasmuch as ρ is a constant of the instrument and is independent of the crystal.

In order to test the truth of the results we have arrived at, a Nicol's prism, the azimuth of which could be read off by a divided circle to 3, was mounted on the side of the table opposite to that on which the elliptic analyser was, in such a position that, when the angle of incidence was the same as before, the light was reflected along the tube containing the Nicol It was found that the light reflected from the surface could be reduced to a minimum but could never be completely eclipsed The minimum was very small but quite perceptible This might be due to the fact that the light was not homogeneous But further experiments showed that, even with orange and ruby glasses there was a perceptible minimum Next the incidence was increased and diminished in succession by 5°, and the same result obtained We, therefore, conclude as before, that the light was slightly elliptically polarised

VIII *Discussion of the Determination of the Retardation of the Quarter Wave Plate*

We have now to inquire into the discrepancy of the values of ρ, which have been found before and after polishing, viz —$\rho = 111° 14'$ and $\rho = 116° 37$

Now, $\cos \rho$ is the ratio of the tangents of two angles, one of which is very nearly 90° Consequently, a very small error in this last angle will produce a considerable change in the value of $\cos \rho$ We therefore come to this conclusion, that it is not a good plan to determine the instrumental constant ρ by reflexion from Iceland spar, because the light reflected is so slightly elliptically polarised To determine more accurately the value of ρ, a steel plate was substituted for the crystal At first, white light was used as before, but the image was found to be strongly coloured The difference in readings for blue and red rays was several degrees After trying a spectroscope and coloured glasses, a ruby glass was selected as the best Then the observations recorded in Table V were made

Table V —Observations with a Steel Plate Mean Temp 17° C.

r	r'	R	R
20 468°	74 705°	89 296°	5 297°
20 515	74 661	89 082	4 885
20 529	74 834	88 917	5 097
20 495	74 117	88 978	5 286
20 692	74 685	89 160	5 805
20 277	74 135	89 094	5 190
20 520	74 282	89 214	5 635
20 527	74 767	89 235	5 400

Table VI.

ϖ.	tan ϖ.	cos ρ.	ρ.	I.
17° 39′ 5	0 3184	0 14595	81° 36′	47 296°
17 43 0	0 3195	0 14063	81 55	46 983
17 52 0	0 3223	0 14797	81 30	47 007
17 57 0	0 3239	0 15006	81 22	47 132
17 46 5	0 3206	0·14814	81 29	47 232
17 50 5	0 3218	0 14768	81 30	47 112
17 52 0	0 3224	0 15355	81 10	47 424
17 38 5	0 3181	0·14999	81 22	47 317

From Table V we may find with what accuracy we can work with the elliptic analyser.

The greatest difference between the mean and a single observation is 0·003, so that the error of determination of tan ϖ is less than a percentage. The mean value of I is 47·188°, and the greatest error 0·236° or 14′. The mean value of ρ is 81° 29′, and the greatest error is somewhat less than 26′. Let d be the space retardation measured in air for wave-length λ, then—

$$\rho = 2\pi d/\lambda.$$

Thus the error in the determination of ρ expressed in wave-lengths is—

$$\delta d = \lambda \frac{\delta\rho}{360} = \lambda \frac{26}{21600} = \frac{\lambda}{831}.$$

As in the experiments tan ϖ = 0 3209, this is very nearly the case in which the accuracy of the analyser has been determined by Professor Stokes. The present results agree well with his limits of accuracy.

"The mean error of single observations amounted to about $\frac{1}{4}$° in the determination of the azimuth of the principal axis, about three or four thousandths in the ratio of the minor to the major axis, and a little more than a thousandth part of an undulation in the determination of ρ"*

We notice that in the mode of expressing ρ in degrees an error will be made more apparent, for the formula $\rho = 2\pi d/\lambda$ shows that the space retardation is divided by the small quantity λ. Thus, expressing the difference 5° between the two sets of observations in wave-lengths, we find that the error of determination of ρ is only about $1\frac{1}{2}$ per cent. of a wave-length.

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* 'Phil. Mag.,' vol. 2, 1851.

The value of ρ which has been determined is for red light, whereas the experiments made on the crystal face were performed with white light. It was not possible to determine ρ accurately for blue rays, as a blue glass absorbed too much light We cannot, therefore, employ the value of ρ as determined by the steel plate to accurately correct the observations made with the crystal. Nevertheless, let us see what the effect of the substitution will be.

We have
$$\cos 2\varpi = \frac{\sin (r'-r)}{\sin (\mathrm{R}'-\mathrm{R})},$$

and
$$\cos \rho = \frac{\tan (r'-r)}{\tan (\mathrm{R}'-\mathrm{R})}.$$

Since ρ is supposed known, eliminate the smaller quantity $\mathrm{R}'-\mathrm{R}$, and we find
$$\sin 2\varpi = \cos (r'-r) \sin \rho. \quad . \quad . \quad . \quad . \quad . \quad (\text{A.})$$

This formula shows that if there be any change in the ratio of the axes there must be a change in the value of $r'-r$. Looking at the Tables I, III, we see there is such a change.

Using the last written formula and the mean values of $r'-r$ from Tables I, III, we find that for a natural face
$$\tan \varpi = 0{\cdot}03552,$$

and for the polished face
$$\tan \varpi = 0{\cdot}02789.$$

Thus, even when the value of ρ for red light as determined by the steel plate is used, we find that the numerical results indicate that the *character* of the change produced by polishing remains the same.

In the case of the natural face, taking 12 observations of $r'-r$ from Table I, the greatest deviation from the mean is less than 9′, leading by formula A to a deviation from the mean of less than 5 per cent. In the case of the polished face, the greatest deviation will be, according to Table III, about 10 per cent.

The value of ρ has been determined for only one colour, and consequently we cannot calculate its value accurately for light of mean wave-length, so as to be able to compare the value of ρ obtained from the steel plate with the value of ρ obtained from observations with the crystal. Let us, however, examine in a general way what the effect of change of wave-length on the value of ρ is.

Let t be the thickness of the selenite plate, μ_x, μ'_x the ordinary and extraordinary indices for a line x of the spectrum.

Then
$$\rho_x = \frac{2\pi}{\lambda_x}\{\mu_x - \mu'_x\}t,$$

and for a line y,
$$\rho_y = \frac{2\pi}{\lambda_y}\{\mu_y - \mu'_y\}t.$$

Consequently,
$$\frac{\rho_y}{\rho_x} = \frac{\lambda_x}{\lambda_y}\,\frac{\mu_y - \mu'_y}{\mu_x - \mu'_x}.$$

Now λ decreases from the lines A to E, and varies by considerably over 30 per cent. The variations in the difference $\mu - \mu'$ are very much less if any analogy holds with Iceland spar and quartz, for which the variation is less than 4 per cent.* Both causes tend to increase ρ as the wave-length diminishes. Thus, somewhere about D the retardation is 90°, and will approximate to the value derived from Table I for white light, as we approach E.

This discussion shows us that while the steel plate affords a more accurate means of determining ρ for light of given refrangibility, yet ρ is to a large extent a purely instrumental constant, such that its value has no effect on the *character* of the results, and, when we consider that white light was used in the previous experiments, the value of ρ determined by Tables I, III, would seem to be confirmed by the steel plate observations. We therefore accept the results of Tables I—IV as determining the alteration in the polarisation of the reflected light.

IX. *Statement of Results.*

The results of Tables I—IV are brought together in Table VII for the sake of future reference and comparison, so as to exhibit a synoptical view of the final result up to this stage of the work.

Table VII.

	tan ϖ.	I.	1st Angle.	2nd Angle.
Before polishing ..	0·03345	108·068°	105° 6′ 29″	74° 58′ 35″
After polishing ..	0·02517	107·819	105 5 37	74 58 44
Difference	−0·00828	−16′ 8″	−52	+9

X. *Variation of Surface State of a Polished Crystal with the Time.*

It is a point of importance to determine if the state of the surface of a *polished* crystal is so permanent that it does not alter with the time, for otherwise the experiments made would not have very great value unless the time elapsed since polishing were specified. I found that preceding experimenters had made investigations on this point in regard to a few bodies, and had come to the conclusion that the surface did not alter with the time. Thus Seebeck found such a result for some glass experimented upon,[*] and Sir John Conroy has proved that in the case of metallic surfaces the surface state is a fairly permanent one, not being destroyed by contact with a liquid or a considerable amount of rubbing with a chamois leather.[†] It therefore naturally occurred to me to examine this question for Iceland spar.

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* Rudberg, 'Poggendorff, Annalen,' vol. 14, p. 45; Glazebrook, 'Phil. Trans.,' 1879.

For this purpose a simple analyser consisting of a Nicol and a graduated circle was set up on the side of the table opposite to the elliptic analyser, and placed in such a position that the light could be reduced to a minimum. The observations taken are recorded in Table VIII.

Table VIII.

Date.	Temp.	No of readings	Mean reading of 1st vernier	Mean reading of 2nd vernier	Mean of verniers.
Dec. 8–11	9°5 C	40	21° 10′ 9	201° 20′ 5	111° 15′·7
Jan. 20 ..	9 7	60	21 11 76	201 21 6	111 16·68
	9·8	60	21 11 93	201 20 8	111 16·36

Thus there is in the period of six weeks no time variation of a *polished* surface, for the differences between the means are quite within the limits of experimental error. Also this result is confirmed by the general character of the observations made with the elliptic analyser, which often extended over some weeks, during which no change was noticed.

XI. *Variation of Surface State of a Polished Crystal with Change of Temperature.*

An attempt was made to secure greater accuracy by altering the arrangement of the parts of the elliptic analyser. For this purpose the screw fixing the vertical stem was loosened, and the circular rim rotated through two right angles about the stem as axis The Nicol was unscrewed from its collar, and the cell containing the quarter wave plate from the disk. The quarter wave plate was now connected with the collar and the Nicol with the disk. In this mode of arrangement, therefore, the *Nicol* moves in azimuth, carrying the quarter wave plate with it, while the quarter wave plate has likewise an independent motion in azimuth.

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[*] 'Poggendorff, Annalen,' vol 20.

[†] 'Roy. Soc. Proc.,' vol 31, 1881.

After taking 700 readings I became convinced that the accuracy of this mode of using the analyser was less than previously. A subsequent investigation of the theory, made by tracing the surface locus of a point whose coordinates represented the intensity of the transmitted light, and the azimuths of the Nicol and quarter wave plate, seemed to confirm this conclusion

It is well known that in the case of some bodies the application of heat considerably influences the mode in which they reflect light. Thus, in the case of flint glass, Sir David Brewster produced as great an alteration as 9° in the polarising angle by varying the temperature.* To test the alteration in the case of Iceland spar, a small tray filled with ice was placed on the top of the crystal, all the rest of the apparatus being carefully protected, especially the woodwork. I found that, leaving the quarter wave plate and Nicol untouched, no perceptible effect was produced by lowering the temperature of the crystal 8°, and also that no difference could be detected when observations were taken. I conclude that in the case of Iceland spar for moderate ranges the effect of temperature is insensible.

XII *New Series of Observations.*

Since the change in the mode of using the elliptic analyser described in the last section had diminished the accuracy of the observations, it was considered best to revert to the original arrangement of the parts of the analyser. The instrument was therefore reversed The screw which fixed the stem of the analyser was unloosened, and the disk rotated through two right angles about the stem as axis. The tube was set to point in the same direction as before by means of the diaphragms and screens. The quarter wave plate was unscrewed from the collar and the Nicol from the disk. The quarter wave plate was now connected with the disk and the Nicol with the collar. Thus the arrangement of the parts of the analyser was now exactly like that described in Section II.

Since the quarter wave plate has been unscrewed, we must no longer expect the values of I to be the same as in Table IV. There will be a constant difference between the preceding series of values and those which follow, because the arbitrary zero from which I is measured is now changed. For these reasons it is necessary to take a new set of observations to serve as a standard of reference.

Sir John Conroy has attempted to determine the effect of polishing a crystal of Iceland spar by observing with a simple analyser the light of a lamp reflected from the surface immersed in water.* With a view to a comparison of my own work with his results, a second Argand burner was placed close to the former. The brass table was unclamped and rotated till the face of the crystal was in such a position that the incident light was reflected to the opposite side of the table to that on which the elliptic analyser was placed. The reflected light was observed with a simple analyser consisting of a Nicol, the azimuth of which could be determined by means of a divided circle and verniers to 3′.

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* 'Phil. Trans.,' 1815.

The observations made with the elliptic and simple analysers are given in Tables IX and X respectively.

Table IX.—Second Series of Observations with the same Polished Face (Elliptic Analyser)

r′	*r*	R′.	R.	I
136 801°	43 732°	133 320°	42 164°	87·742°
136·825	43 817	133 591	42·212	87 901
136 779	43 557	133·555	42·200	87 877
136·940	43 311	133·431	42 066	87·748
136 980	43 345	133 149	41 840	87·494
136 410	43 105	133 307	42·095	87·701
137·070	43·845	133·431	41·792	87 611
136·903	43 452	133·137	41·769	87 453
137·131	43·782	133 085	41·657	87 371
136·878	43·476	133 294	41·945	87 619
137·406	43 946	133 277	41 966	87·621
136 566	43·860	133·220	42·025	87 622
136 767	43·503	133·427	41·762	87 595
136·974	43·524	133 591	42 011	87 801

Table X.—Second Series of Observations with the s
Face (Simple Analyser).

	Mean of ten readings of 1st vernier	Mean of ten readings of 2nd vernier
	356° 5′·0	176° 13′·8
	355 57·4	176 6 1
	355 59·4	176 8·2
	356 2·9	176 12 1
	355 59·4	176 8·6
Mean ..	356 0 82	176 9·76

Mean of 100 = 266° 5·3′.

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* 'Roy. Soc. Proc.,' Feb., 1886.

Taking the mean values of r', r, R', R from Table IX, the elliptic analyser gives

$$\cos 2\varpi = \frac{\sin (r'-r)}{\sin (R'-R)} = \frac{\sin 93{\cdot}340}{\sin 91{\cdot}380} = 0{\cdot}998591,$$

whence $\varpi = 1^\circ\ 31{\cdot}2'$, and we have for the values of the quantities which fully determine the nature of the reflected light

$$\tan \varpi = 0{\cdot}02625, \qquad \text{and} \qquad I = 87{\cdot}654^\circ.$$

The previous experiments, the results of which are recorded in Table VII, give

$$\tan \varpi = 0{\cdot}02517, \qquad \text{and} \qquad I = 107{\cdot}819^\circ$$

The reason of the difference of the values of I is the change of the index error. In order to compare the values of I found in a subsequent part of the paper with those already obtained, we must therefore subtract 20·165°. The changes in tan ϖ may be ascribed to two causes—error in resetting the instrument so that it was not exactly in its former position, and error of experiment. It should be noted that the difference produced by the reversal of the instrument is not at all comparable with the difference produced by polishing.

In the case of the readings taken with the simple analyser, the mean reading will correspond to the azimuth of the major axis of the ellipse, determined by the elliptic analyser. Thus from Table X

$$I' = 266^\circ\ 5{\cdot}3'$$

XIII *Determination of the Effect of Rotation of the Crystal Face in its own Plane.*

It was suggested that the setting of the crystal was such as to allow a rotation of the face in its own plane, which might possibly be the cause of the differences hitherto observed. A wedge of 4° 27′ angle was constructed, and upon this the crystal was placed. So delicate was the mode of setting by screens that, even with the labour of hours, I was unable to turn the crystal into such a position that the reflected ray emerged through the pinholes. I therefore took a thin sheet of paper and gently wore its surface away so as to obtain a fine wedge which was drawn under a corner of the crystal. By this means the reflected ray with some trouble was rendered visible. The observations are divided in the following table into sets of two or three. Each set was taken after the crystal had been taken off the wedge and replaced upon it in position by means of the screens. By this means the accuracy of setting can be judged.

Table XI.—Observations with Polished Face

r'.	Mean r'.	r.	Mean r.	R′	Mean R′.	R	Mean R.
137·120°		43·050°		133 041°		41 621°	
137·404		42·727		133 269		41·319	
137·357		43·080		133·421		41 536	
	137·297°		42·052°		133 244°		41 492°
137·307		43 104		133 331		41 556	
137·075		42·875		133·267		41 332	
137·242		43 062		133 135		41 474	
	137·208		43·014		133 244		41·454
137 031		43·289		133 121		41 617	
137 401		43 097		133·556		41 715	
137 336		43 009		133 455		41 479	
	137·256		43·132		133·377		41·613
137·155		42·890		133·035		41·366	
137·382		42·984		133 000		41·231	
	137·268		42 937		133 017		41·298
137·346		43 040		133·441		41·621	
137·255		43·091		133 299		41 812	
137·425		42·921		133 319		41·452	
	137·342		43·017		133 353		41 628
137·106		43 400		133·485		41·869	
137·047		43 257		133·474		41 910	
136 869		43 147		133 338		42 034	
137·014		43·165		133·525		42 127	
137 089		43·347		133 562		41·712	
137·155		43 124		133 250		41 549	
137 264		43·055		133·381		41·886	
	137·078		43·214		133·430		41·872
137·067		42 809		133·612		42 048	
136·954		42 938		133 489		41 665	
137·020		42·821		133 366		41 834	
136·992		42·977		133·419		41 697	
137·064		43 104		133 228		41 681	
136·730		42·859		133 124		41 704	
137·045		42·731		133·266		41·424	
137 179		42 895		133·216		41 719	
137 006		43·075		133·217		41·636	
	137 078		42 912		133 326		41·712

The mean of the 960 observations of Table XI gives—

$$r' = 137{\cdot}168^\circ, \qquad r = 43{\cdot}031^\circ,$$
$$R' = 133{\cdot}321^\circ, \qquad R = 41\ 656^\circ.$$

Thus
$$I = 87\ 488^\circ.$$

But Table IX gives for the mean value of I—

$$I = 87\ 654^\circ,$$

a difference from the above of 0·166

Table VII shows us that the effect of polishing is to decrease I from 108·088° to 107·819°, a diminution of 0·269°. Since then a rotation of 4° 27′ produces an increase of 0·166; to produce an effect equal to that of polishing by rotation, the crystal face would have to be rotated in its own plane 8°. No such error of setting was possible.

Again,

$$r'-r = 94{\cdot}137^\circ, \qquad \text{and} \qquad R'-R = 91{\cdot}665^\circ.$$

The last reading is of an altogether different magnitude to what has hitherto occurred.

A template was constructed and used in future observations to test the accuracy of setting. It was found that the setting by the screens was always correct on afterwards being examined by the template, and when it was out of adjustment by the screens the template did not fit. These three grounds seem to be more than sufficient for the rejection of the hypothesis of a rotation of the crystal face in its own plane.

It may be of some interest to calculate the value of the change in the ratio of the axes produced by a rotation of 4° 27′. We therefore calculate it—

Thus $$\cos 2\varpi = \frac{\sin 94{\cdot}137}{\sin 91{\cdot}665},$$

whence $$\varpi = 1^\circ\ 53{\cdot}6',$$

and $$\tan \varpi = 0{\cdot}03305.$$

The value before rotation was

$$\tan \varpi = 0{\cdot}02655.$$

XIV. *Determination of the Effect of Repolishing the same Face of the Crystal. Investigation of the Errors of Means of a Series of Observations with an Elliptic or Simple Analyser. Comparison of the Relative Accuracy of the Elliptic and Simple Analysers.*

Much of the value of the observations on polishing must depend on the fact whether the crystal can be polished in the same manner, that is, polished and repolished so as to obtain the same results. The crystal was, therefore, submitted to the same process of polishing as has been described already in Section VI. The observations with the same face thus repolished are recorded in Table XII.

Table XII —Observations with a Repolished Face (Elliptic Analyser)

		Mean		Mean		Mean		Mean	Mean
	r	*r'*	*r*	*r*	R	R	R	R	I
No 1	136 586°		43 256°		133 879°		42 035°		
	136 846		43 276		133 265		41 680		
	136 554		43 177		133 652		42 061		
	136 794		43 090		133 478		41 915		
	136 884		43 145		133 372		41 941		
	136 619		43 370		133 578		41 779		
	136 455		43 325		133 480		42 051		
	136 607		43 585		133 476		41 875		
	136 941		43 291		133 269		41 979		
	136 872		43 490		133 525		42 217		
	136 846		43 244		133 559		41 876		
	136 841		43 267		133 485		41 977		
		136 737°		43 289°		133 501°		41 951	87 726°
No 2	137 080		43 196		133 352		41 699		
	136 705		43 305		133 223		42 051		
	136 865		43 511		133 456		42 335		
	136 770		43 227		133 359		41 832		
	136 731		43 064		133 510		41 794		
	136 975		43 257		133 638		41 892		
	136 819		43 120		133 277		41 949		
	136 796		43 176		133 049		41 854		
	136 890		43 290		133 402		41 835		
	136 594		43 338		133 542		42 036		
	136 621		43 577		133 491		42 165		
	136 604		43 670		133 221		41 989		
		136 787		43 311		133 381		41 952	87 066
No 3	136 589		43 562		133 311		42 361		
	136 722		43 399		133 780		42 044		
	136 705		42 967		133 637		42 105		
	136 657		43 402		133 457		42 165		
	136 722		43 121		133 575		42 306		
	136 585		43 446		133 536		41 857		
	136 850		43 300		133 259		41 937		
	137 091		43 435		133 326		41 719		
	136 740		43 329		133 485		42 062		
	136 824		43 234		133 410		42 064		
	136 774		43 306		133 437		41 972		
	136 672		43 416		133 327		41 875		
		136 744		43 326		133 461		42 039	87 750
No 4.	136 772		43 640		133 290		41 894		
	136 929		43 174		133 542		41 936		
	136 732		43 289		133 414		41 937		
	136 645		43 557		133 565		41 900		
Means for 1280 readings		136 758		43 320		133 440		41 975	87 712

The observations in Table XII made with the elliptic analyser are divided into sets of 12, for our object is not only to determine what is the change produced by repolishing, but also to discover the error of a mean of 12 sets of observations with a view to fixing the accuracy of working with the elliptic analyser, and also the errors of the means of the sets of observations recorded in Tables I—IV. For convenience of comparison the means are exhibited together in Table XIII

Table XIII.—The Means of Observations with a Repolished Face (Elliptic Analyser)

	$r'-r$.	$R'-R$	$\sin(r'-r)$	$\sin(R'-R)$	$\cos 2\varpi$.	ϖ.	$\tan\varpi$.
No. 1..	93·448	91·550	0·998190	0·999634	0·998555	1° 32′ 41	0·02689
No. 2..	93·476	91·429	0·998160	0·999689	0·998471	1 35 07	0·02766
No 3..	93·418	91·422	0·998221	0·999692	0·998529	1 33 25	0·02713
Mean..	93·447	91·467	0·998190	0·999672	0·998518	1 33·57	0·02723

We now proceed to consider the accuracy of the sets of observations with the elliptic analyser Table XIII shows that the greatest deviation of a single set of 12 observations from the mean is less than 1·6 per cent. for the ratio of the axes. The best set differs from the mean by under ¼ per cent Again, Table XII shows that the greatest deviation from the mean for I is 0·046° or under 3′, whereas the least deviation from the mean is under 1′. We take, then, the upper limits as the errors of observations. Let us now consider the effect produced by repolishing. Before repolishing, the calculation following Table IX shows that—

$$\tan\varpi = 0{\cdot}02655$$

After repolishing, Table XIII shows that—

$$\tan\varpi = 0{\cdot}02723.$$

The difference between the two values of $\tan\varpi$ is 2½ per cent., which may be covered by the limits of errors of observation.

Again, before repolishing—

$$I = 87{\cdot}654.^\circ$$

After repolishing, Table XII shows that—

$$I = 87{\cdot}712^\circ.$$

There is thus a difference of 0·058° or 3·48′, which is covered by the limits of errors of observation.

Again, Table VII shows that the effect of the first polishing was to *diminish* I from 108 088° to 107 819°, a *diminution* of 0 269°, but the effect of the second polishing has been to *increase* I from 87 654° to 87 712°, an *increase* of 0 058° Thus the effect of the polishings has not been cumulative so as to cause the original deviation of polishing to increase still further on second polishing On the contrary, I has approached by a small quantity the value it would have for a natural face The inference may be drawn that the first polishing produced *all* the effect that polishing can bring about, and that the second polishing differs from the first by a quantity which there is some ground for regarding as an error of experiment But if this is so, it may be thought that the values of $\tan \varpi$ the second independent test of a change in the reflected light should also alter in the same manner and sense as those of I have done

Now Table VII shows that the effect of polishing is to *diminish* $\tan \varpi$ from 0 03345 to 0 02517 a diminution of 0 00828 Table XIII and the calculation following Table IX show that the effect of re polishing is to cause $\tan \varpi$ to *increase* from 0 02655 to 0 02723 an *increase* of 0 00068 Thus $\tan \varpi$ changes sympathetically with I

In Table XIV are contained the results of the observations made with the simple analyser, and we have first to investigate the limits of accuracy In regard to single observations there can be no doubt of the correctness of Sir John Conroy s observation that in order to obtain accurate results with observations of this kind, it is necessary to make a large number of observations, and to take their mean * Thus in Table XIV the means of tens of readings are recorded, and for purposes of comparison the means of fifties are tabulated

Table XIV —Observations with a Repolished Face (Simple Analyser)

	Means of ten readings of 1st vernier	Means of 50	Means of ten readings of 2nd vernier	Means of 50	Means of 100
No 1	355 52 2		176° 1 2		
	355 50 3		175 59 1		
	355 57 7		176 5 6		
	355 49 9		175 59 3		
	355 51 6		176 0 7		
		355° 52 3		176° 1 2	265° 56 8
No 2	355 59 5		176 6 1		
	355 53 6		176 1 6		
	355 58 3		176 7 8		
	355 54 9		176 4 6		
	355 50 5		175 59 6		
		355 55 3		176 3 9	265 59 6

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* Roy Soc Proc Feb, 1886

Table XIV—*continued.*

	Means of ten readings of 1st vernier	Means of 50	Means of ten readings of 2nd vernier.	Means of 50.	Means of 100.
No. 3......	355° 49 5′		175° 58 6′		
	355 56 8		176 5 6		
	355 46·0		175 54 9		
	355 51·7		176 0·1		
	355 51·5		176 0·6		
		355° 51·1		176° 0·0′	265° 55·5
No. 4......	355 53·8		176 3 3		
	355 51·7		176 0·5		
	355 49 7		175 58·7		
	355 50·6		175 59·4		
	355 46·6		175 55·8		
		355 50·5		175 59·5	265 55 0
No. 5......	355 50 8		176 0·3		
	355 54 9		176 4·4		
	355 51 8		176 0·8		
	356 2·4		176 10·8		
	355 54 5		176 4·0		
		355 54 9		176 4·06	265 59·5
				Mean of 500 =	265 57 28

The results in Table XIV show a greatest error of 2 32′ from the mean for a 100 readings, and of about 4′ for 10. Before polishing the value of I as given by Table X is—

$$I' = 266^\circ\ 5{\cdot}3'.$$

After polishing, Table XIV gives—

$$I' = 265^\circ\ 57{\cdot}28'.$$

There is thus a difference of 8′. This is covered by the larger limit above, but not by the lower. This is not surprising when the errors and fluctuations of single observations are taken into account, and also the number of observations.

The parallelism of the polished and repolished faces was tested by means of the lines drawn round the sides. As these lines gave no perceptible inclination, it did not seem worth while to measure the angles with a spectrometer, since the differences caused by repolishing are so small.

Thus, we come to the conclusion that the repolishing produced little, if any, alteration in the nature of the reflected light, and that this alteration is very small compared with the change produced by polishing a natural face. Also, this inference is supported by two data as regards the elliptic analyser, and by the results obtained with the simple analyser.

Sir John Conroy[*] has come to a different conclusion. "It did not seem worth while to make any further experiments with artificial surfaces, as it seemed certain the results would be untrustworthy."

Conroy's experiments were made with the surface immersed in water to secure a greater rotation. I am not sure that the greater rotation would compensate for the loss of light which must ensue when reflexion takes place at the surface of two media of nearly the same density and refractive index.

Another cause of error has been indicated by Conroy. There was some uncertainty whether the face was polished parallel to itself. Though it would not altogether account for the large changes observed by Conroy, yet I found in the case of a crystal, with which I performed a first set of experiments for two months, that a perceptible alteration of the inclination of the face was sufficient to reduce the elliptically polarised light to plane polarised. Thus as regards my own experiments such a change of inclination would have been quite fatal, and I abandoned the crystal.

There is another point which is worthy of attention An extreme ill-luck seems to have befallen those who have had their crystals polished by others. Thus Seebeck[†] found that in the case of a glass polished by an artist there was a difference of 28 5′, but that he himself could polish it so that the difference was only 2·9′. I think most likely this may be the probable cause of the large alterations observed by Sir J. Conroy

XV *Experiments with a Cleavage Face parallel to the Repolished Face.*

To make the whole series of experiments as conclusive as possible, I now attempted to make a series of experiments with a natural face as at the beginning of the paper. Unfortunately the crystal did not cleave very readily. In splitting the base of the crystal came into two or three pieces, which were fixed on to it again as accurately as possible in their former position. As the sides of the crystal also came off, the inclination of the face to the repolished face could not be determined. There was some doubt as to whether the face of the crystal was in the same absolute position. The surface was not very good, being somewhat broken, and as the reflexion took place from a part of the crystal nearer an edge, it had to be placed so much on one side of the brass table that the template could not be used. Then the observations recorded in Table XV were taken.

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[*] 'Roy. Soc. Proc.,' Feb., 1886.

[†] 'Poggendorff, Annalen,' vol. 20

Table XV —Second Series of Observations with a Natural Face

	r	r	R′	R
	137 020°	42 589°	134 230°	42 682°
	136 935	42 804	133 974	42 539
	137 105	43 015	133 882	42 404
	136 819	43 032	133 994	42 581
	137 046	42 496	134 302	42 510
	136 884	42 744	134 325	42 652
	136 940	42 990	133 781	42 604
	137 216	42 674	134 091	42 556
	136 900	42 971	133 921	42 710
	136 781	42 767	134 317	42 630
	136 961	42 791	134 337	42 636
	137 083	43 030	134 285	42 513
Means .	136 974	42 825	134 122	42 594

Thus we find that $$I = 88\ 358^\circ,$$

and $$\tan \varpi = 0\ 03368$$

We have now to compare these results with those recorded in Table VII, taken some fifteen months previously Originally we had—

$$\tan \varpi = 0\ 03345$$

whereas the present value is—

The agreement is very close, and, as might be expected from a comparison of Table VII with the results of Table IX, the latter number is somewhat larger than the former on account of the resetting of the instrument Again, the first series of observations show that the effect of polishing was according to Table VII to change I from 108 088° to 107 819°, a decrease of 0 269°, whereas the second series of experiments show that the result of repolishing has been according to Tables XII and XV to change I from 88 358° to 87 712°, a decrease of 0 646° This result is correct as regards sense, but not so satisfactory as regards magnitude It must, however, be remembered that the breaking up of the base of the crystal would most probably have an influence on the value of I,

We are now in a position to sum up the results of this investigation The process of polishing a natural face of a crystal of Iceland spar with emery and rouge does most certainly alter the state of the surface This alteration is evinced by a change both in the ratio of the axes and in the azimuth of the major axis of the elliptically polarised light, and was observed in the case of two different crystals which were made the subject of experiment.

Since the light reflected from the surface of the crystal is exceedingly nearly plane polarised, the absolute value of the change in the ratio of the axes is small; but the relative change is considerable, for tan ϖ is altered by polishing from 0·0334 to 0·0252. Also the change in the azimuth of the major axis is not very large.

As regards the disturbing causes, it is found that temperature and time do not cause any very perceptible alterations in the surface state of a polished crystal

The experiments prove a result unnoticed by Seebeck, that an emery-rouge polished surface gives perfectly concordant results on repolishing, and in this respect is quite as satisfactory as the chalk-polished surface recommended by him. This conclusion is supported both by the elliptic and simple analysers. And in general the results of the paper tend to confirm the views of Seebeck rather than those of Sir J. Conroy, for Seebeck in his paper prefers polished surfaces because of the liability of the natural surface to tarnish.

In conclusion, my best thanks are due to Mr. Glazebrook for his advice, and to Professor J. J. Thomson for placing at my disposal a room and apparatus in the Cavendish Laboratory.

"Further Experiments on the Distribution of Micro-organisms in Air (by Hesse's Method)." By PERCY F. FRANKLAND, Ph.D., B.Sc., F.C.S., and T. G. HART, A.R.S.M. Communicated by Prof. FRANKLAND, D.C.L., F.R.S. Received November 22,—Read December 9, 1886.

[PLATE 3.]

In a previous communication entitled "The Distribution of Micro-organisms in Air," a number of experiments have been recorded by one of us on the relative abundance of microbes in the air of various places and of the same place at different times. The numerical determination of the aërial micro-organisms in these experiments was made by means of Hesse's apparatus, the method of using which was there fully described. Since the publication of the above experiments we have been extending our investigations by means of this method, and the results which we have obtained form the subject of the present communication.

In addition to the determination of the number of micro-organisms in a given volume of air by means of Hesse's process, we have also, as before, determined in each case the number of microbes falling on a given horizontal area in unit of time, by the exposure of small dishes containing sterile nutrient gelatine, as previously described.

During the hot weather we have experienced considerable difficulty in working with Hesse's tubes, which are very liable to melt in transport and when exposed to the sun; to obviate this we have made a practice on hot days of surrounding the outer surface of the tube with a coating of bibulous paper saturated with water, and this envelope was again covered with a coating of white tissue paper to prevent the former drying too rapidly. Owing to this precaution we have scarcely ever lost a tube even on the warmest days.

The greater number of our observations have been made on the roof of the Science Schools, South Kensington, which thus form a continuation of those already recorded. We have also made further experiments in Hyde Park, the Brompton Hospital for Consumption, the Natural History Museum, and in the garden of the latter closely adjoining the traffic in the Exhibition Road, South Kensington.

I. *Roof of Science Schools, South Kensington.*

The samples of air were here collected at a height of about 60 feet above the ground in the manner previously described. The particulars of the experiments are recorded in the following table (Table 1).

These experiments, taken together with those already published, form a continuous series, excepting a few breaks, from January to the end of October of the present year, and serve to illustrate the changes which take place in the prevalence of aërial micro-organisms according to the seasons.

In order to render these results more readily intelligible, we have also expressed them by means of a curve in the accompanying diagram (Plate 3), in which the ordinates represent the number of micro-organisms found in 10 litres of air, whilst on the horizontal axis the dates are marked off. Below, on the same diagram, the temperature of the air at the time of experiment is recorded.

From this diagram it will be seen that, although the number of micro-organisms in the air frequently undergoes great changes from one day to another, yet the general tendency is for the number to follow the temperature. Thus, on taking the average of the results obtained in each month, the following sequence is arrived at:—

1886.	Average temperature at time of experiments.	Average number of colonies obtained from 10 litres of air by Hesse's method.
January....	3·5° C.	4
March	6·9	26
May..	13·4	31
June	20·2	54
July	23·6	63
August	18·3	105
September .	12·9	43
October....	12·5	35

From the above it will be seen that it is during the hottest months of the year—July and August—that the largest number of micro-organisms is present in the air. In September the number underwent a great reduction, which was further continued in October.

II. *Experiments in Interior of Buildings.*

In Table No. II we have recorded a number of experiments which we have made in the interior of buildings, viz., in the Hospital for Consumption, Brompton, in Burlington House, in the Natural History Museum, in the Chemical Laboratory of the Science Schools, South Kensington, as well as in a barn and cowhouse in the country.

The results of the above experiments fully substantiate the previous observations made by one of us, that in the interior of buildings, &c., the number of organisms present in the air is almost wholly dependent upon whether the latter is in a disturbed state or not, and that when undisturbed the number is generally considerably less than in the open air, whilst in crowded rooms the number rises enormously. Thus in the Hospital for Consumption, when only a few persons were moving about the ward, the air was remarkably free from microbes, the number increasing somewhat when the number of people in the room increased, but even then the number fell very far short of that in the crowded rooms of the Royal Society during the conversazione, or in the Natural History Museum on Whit Monday, whilst the greatest number which we have ever recorded was found in a barn in which the operation of thrashing was going on, the number of micro-organisms falling on the square foot in one minute amounting there to upwards of 8000.

III. *Miscellaneous Open-Air Experiments.*

In Table III we have recorded the results of some experiments made in the open air at other places than the roof of the Science Schools. This table includes, in the first place, a comparison between the number of micro-organisms present in the air of Hyde Park, the roof of the Science Schools, and the entrance to the latter in the Exhibition Road respectively, the experiments in these three places being all made on the same day It will be seen that the air in Hyde Park contained markedly less than either of the other two, and that the air in the Exhibition Road in which a large amount of traffic was going on at the time was considerably richer in micro-organisms than the air on the roof.

Then follow several experiments made in the garden of the Natural History Museum in the immediate vicinity of the Exhibition Road. In every case, excepting one, the air was exceedingly rich in micro-organisms, as was to be anticipated; whilst on the occasion when the number of organisms was small, the wind, which was very gentle, was not blowing from the road but over the grass of the garden which was damp at the time

The above experiments are intended to form a supplement to those already published by one of us, in which the same method of investigation was pursued. In the course of these experiments we have found that the results obtained with Hesse's apparatus are liable to considerable inaccuracy when the latter is employed in a disturbed atmosphere, more especially when the aerial currents are irregular in direction. This source of error has been fully discussed in another paper by one of us, and a new method of examining air for micro-organisms, in which this difficulty is overcome, has been devised and its accuracy carefully tested.

[*Note.*—Since the communication of the above, we have completed the observations for November and December, with the following results:—

1886.	Average temperature at time of experiments.	Average number of colonies obtained from 10 litres of air by Hesse's method.
November..	9·4° C.	13
December..	4·4	20

March 5, 1887.]

Table I —Roof of Science Schools South Kensington Museum

Date 1886	Conditions of experiment	Number of colonies obtained from given volume of air	Number of colonies from 10 litres of air (calculated)	Number of micro organisms falling on 1 horizontal square foot per minute
June 7th 4 15—5 15 P M	Wind N E by N *moderate but variable* and irregular in direction blew into tube during the 11th litre Sunshine roads dry except where watered 21 C	74 in 12 lits	62	54
June 18th 4.15—5 15 P.M	Wind N varying to N W and N E *slight* No rain on previous day but few drops during experiment Roads dry except where watered 10° C	12 in 12 lits	10	85 (1) 82 (2)
June 21st 5—6 P M	Wind W by N W *moderately strong* but *constant* Roads dry except where watered No considerable rain since night of June 18th Cloudy 14 C	27 in 12 lits	23	107 (1) 146 (2)
June 22nd 3—4 P M	Wind W by N W *rather stronger than on previous day* Only small quantity of rain since previous day Slight sunshine 18·5 C	53 in 12 lits	44	153
June 24th 4—5 P M	Wind W by S W *very strong and gusty* Much dust blowing about No rain on previous day 22 C	135 in 12 lits	113	1919 (1) 1714 (2)

Table I—*continued*.

Date. 1886.	Conditions of experiment.	Number of colonies obtained from given volume of air.	Number of colonies from 10 litres of air (calculated).	Number of micro-organisms falling on 1 horizontal square foot per minute.
June 25th. 3—4 P.M.	Wind W., *moderately strong* Roads watered, but rather dry at the time No rain on previous days. 23° C.	114 in 12 lits.	95	468
June 26th. 12—1 P.M.	Wind S. by S.W., *very gentle*. Hot and sultry. Roads watered previously. No recent rain. 25·5° C.	52 in 12 lits.	43	50
June 28th. 3.40—4.30 P.M.	Wind N.E. by E., *moderate but variable*. Roads watered No rain. 21·5.°	49 in 12 lits.	41	133
Ditto. 4.40—5.25 P.M.	Wind *moderate and not quite so variable* as in previous experiment, increased *very* much at 5 12 P M.	53 in 12 lits.	47	188
June 29th. 3—4 P.M.	Wind N E, very gentle and fairly constant. Sunshine, very hot. No previous rain. 24° C.	45 in 12 lits.	38	56
June 30th. 4—5 P.M.	Wind N.E., *very considerable*, and increased very much at end of experiment. No previous rain. 21° C	75 in 10 lits.	75	226

Table I—*continued*

Date 1886	Conditions of experiment	Number of colonies obtained from given volume of air	Number of colonies from 10 litres of air (calculated)	Number of micro-organisms falling on 1 horizontal square foot per minute
July 2nd 4—5 P M	Wind E *gentle*, increased somewhat during experiment No previous rain Sunshine throughout day 24 7° C	51 in 12 lits	43	100
July 3rd 12—1 P.M	Wind N W, but *almost perfectly calm* Roads well watered, but no previous rain Very hot and sultry 26 1°	38 in 12 lits	32	42
July 5th 3—4 P M.	Wind N W, *considerable* No previous rain but roads well watered, some dust seen, however, to be blown about 27 7° C	121 in 12 lits	101	283
July 6th 3—4 P M	Wind N W by W, *very considerable* No previous rain 27 5 C°	78 in 12 lits	65	364
July 7th 1 30—2 30 P M	Wind S W by W, *very slight indeed* No previous rain 25 5° C	55 in 12 lits	46	226

Table I—*continued*

Date 1886	Conditions of experiment	Number of colonies obtained from given volume of air	Number of colonies from 10 litres of air (calculated)	Number of micro organisms falling on 1 horizontal square foot per minute
July 8th 6—7 P M	Wind N W *moderate* No previous rain 22 2° C The number of colonies in the tube is only approximate owing to the growth of a large liquefying colony	(33 in 12 lits)	(28?)	273
July 9th 3—4 P M	Wind N E by E, *strong* No previous rain. 17 5° C	59 in 12 lits	49	143
July 12th 12—1 P M	Wind S W *slight* Considerable rain during preceding night Roads &c quite wet during the morning but drier at time of experiment Slight rain during experiment 21° C	72 in 12 lits	60	88
July 21st 4 18—5.20 P M	Wind S W *very gentle but increasing* considerably at end of experiment when it changed to W by N W and blew much dust about 26° C	129 in 12 lits	108	203 (little wind) 798 (more wind)
July 29th 12 40—1 45 P M	Wind S W *gentle but variable* both in strength and direction No previous rain, but roads well watered 22 5° C	86 in 12 lits	72	P M 180 (1) {1 7 to 1 37 220 (2) {1 30 to 1 45

Table I—*continued*

Date 1886	Conditions of experiment	Number of colonies obtained from given volume of air	Number of colonies from 10 litres of air (calculated)	Number of micro-organisms falling on 1 horizontal square foot per minute
July 31st 12 15—1 5 P M	Wind W *fairly strong but varying in strength* Rain during previous night but roads and pavements quite dry dust seen to be blowing about in road below 18 6° C	108 in 12 lits	90	252
August 3rd 3 45—4 45 P M	Wind N W *gentle but variable in direction* Rain on the morning of previous day Roads watered (the number of colonies obtained in the 10 litres is not reliable and must be too low as the aspirator leaked during a part of the time) 18 3° C	(105 in 10 lits) ?	(105 ?)	126
September 24th 3 30—4.20 P M	Wind N by N W *very gentle* but variable both in strength and direction No rain for some time previously Roads watered pavement dry (result of tube experiment probably too low owing to slight leakage) 13 3 C	(71 in 12 lits) ?	(59)	183 { 3 45 to 4 15 P M 132 { 4 1 to 4 32 P.M
September 25th 6 20—7 15 P M	Wind S W *very gentle* No previous rain but a few drops felt at beginning of experiment Roads watered pavement dry 12 5° C	32 in 12 lits	27	68 74

Table I—*continued.*

Date. 1886.	Conditions of experiment.	Number of colonies obtained from given volume of air	Number of colonies from 10 litres of air (calculated).	Number of micro-organisms falling on 1 horizontal square foot per minute
October 19*th.* 4.58—5.32 P.M.	Wind S.E., *gentle*, constant in direction. Rain during morning; roads wet, roof and pavement nearly dry.	31 in 9 lits.	34	57
October 20*th.*	Wind W. by S W., *gentle*, but increased during experiment. Roads wet, pavement dry. Rain during previous night. 12·8° C.	26 in 10 lits.	26	30
October 22*nd.* 10.44—11.25 A.M.	Wind S.W., changing to W., *very gentle*, but variable Roads wet; roof still wet in places from heavy dew. Foggy morning, which had cleared to sunshine. 9·4° C.	18 in 11 lits.	16	6
October 25*th.* 10.44—11.26 A.M.	Wind E., *very strong and gusty*, but fairly constant in direction. Rain earlier in the morning. Roads wet, pavement dry, roof damp. 11·3° C.	68 in 10 lits.	68	278
October 29*th.* 10.51—11.38 A.M.	Wind S.W., *gentle*, with occasional gusts, fairly constant in direction. Roads and pavement wet. Roof still damp from previous rain. 16·3 C	35 in 12 lits.	29	42

Table II.—Interior of Buildings.

Date. 1886.	Conditions of experiment.	Number of colonies obtained from given volume of air.	Number of colonies from 10 litres of air (calculated).	Number of micro-organisms falling on 1 horizontal square foot per minute.
June 1st. 12.30 P.M.	*Hospital for Consumption, Brompton* ("Richmond" Ward, 8 beds). Windows slightly open at top 17° C.	19 in 10 lits. 5 moulds	19	18
3.30 "	Ditto. More people moving about than in the morning 18·5° C.	34 in 10 lits 6 moulds	34	66
June 9th. 9·20 P.M.	*Burlington House*, Royal Society's Library during Conversazione. 19·5° C	163 in 5 lits. 11 moulds	326	240
10·5 "	Ditto. Room more crowded. 22·0° C.	216 in 5 lits. 14 moulds	432	318
June 10th. 10.15 A.M.	Ditto. Only 3 persons in room, much dust had fallen on furniture, &c. 17° C.	65 in 5 lits. 32 moulds	130	109
June 14th. 4.53—5.40 P.M.	*Natural History Museum*, Central Hall. Very large number of visitors (Whit Monday) and draught from entrance doors. 15·5° C	280 in 10 lits 8 moulds	280	1755
5.45—6.32 "	Ditto. Ditto. 15·0° C.	267 in 10 lits 5 moulds	267	1568
August 20th. 6 P.M.	*Cowhouse*, near Hughenden, Bucks. 3 cows in stalls, milking going on, dish exposed 3 feet from ground.	No tube	..	282

Table II—*continued*

Date 1886	Conditions of experiment	Number of colonies obtained from given volume of air	Number of colonies from 10 litres of air (calculated)	Number of micro organisms falling on 1 horizontal square foot per minute
August 20th 10 A.M	*Barn* near Hughenden Bucks One man thrashing rye with flail doors wide open dish exposed 3 feet from ground	No tube	..	8555
October 15th 4.50—5 35 P M	*North Chemical Laboratory* Science Schools South Kensington About 40 students had been working until half an hour previously Almost empty at time of experiment 16 9° C	30 in 10 lits 16 moulds	30	29
October 16th 2 3—2 35 P M	*North Chemical Laboratory* As it was Saturday only 12 students had been working in the morning and had left about 1 hour previously the laboratory being quite empty 16 9° C	13 in 9 lits 11 moulds	14	13
October 27th 10 59—11 34 A M	*Private Laboratory* Science Schools Windows and door closed 3 persons in room but not moving about 16 9° C	32 in 10 lits 14 moulds	32	9
November 12th 3—4 P M	*Private Laboratory* Science Schools Windows and doors closed 18 6° C	9 in 11 lits 6 moulds	8	7

Table III

Date 1886	Conditions of experiment	Number of colonies obtained from given volume of air	Number of colonies from 10 litres of air (calculated)	Number of micro organisms falling on 1 horizontal sq arc foot per minute
June 4th Noon	*Hyde Park* Wind N E, *moderate* Sunshine Roads dry 12 5° C	—	—	37
1 P M	Ditto Wind increased in strength 13 5° C	—	—	78
June 7th 12 45 P M	*Hyde Park* Wind N W *moderate* but irregular Sunshine Roads dry grass damp 18 5° C	22 in 12 lits 7 moulds	18	16
4 15—5 15 P M	*Roof of Science Schools* South Kensington Wind N E by N *Moderate* but variable in direction blowing towards tube during 11th litre Roads partially watered 21° C	74 in 12 lits 7 moulds	62	52
6 30—7 15 P M	*Entrance to Science Schools* in Exhibition Road. Much traffic in street causing dust 18 C	113 in 12 lits 27 moulds	94	945
June 8th Noon	*Natural History Museum Garden* adjoining Exhibition Road Wind S Roads dry 19 C	—	—	499
4 P M	Ditto Ditto Number of people passing along road greater than in morning 19° C	665 in 12 lits 239 moulds	554	499

Table III—*continued.*

Date. 1886.	Conditions of experiment.	Number of colonies obtained from given volume of air.	Number of colonies from 10 litres of air (calculated).	Number of micro-organisms falling on 1 horizontal square foot per minute.
June 8th. 5 P.M.	*Natural History Museum Garden*, adjoining Exhibition Road. Wind S Number of people passing along road greater than in morning. 18° C.	261 in 8 lits. 5 moulds	309	1255
June 9th. 12 noon.	*Natural History Museum Garden* Wind S, but not quite so strong as on previous day. Traffic in road less. 17 5° C.	158 in 12 lits. 17 moulds.	132	262
June 10th. 4—4.45 P.M.	*Natural History Museum Garden.* Wind N.W., *very slight and gentle.* Heavy rain during greater part of morning. Grass damp, roads wet, pavement dry. Wind blowing across grass and not from road 15 5° C.	20 in 11 lits. 3 moulds	18	81
August 20th. 11 A.M.	*Hughenden*, Bucks. Naphill Common, covered with heather and fern. Sunshine. Wind very slight. No rain since previous day.	—	—	78 ($\frac{8}{13}$ moulds)
6.30 P.M.	Ditto. Garden, grass all round.	—	—	91 ($\frac{1}{4}$ moulds)

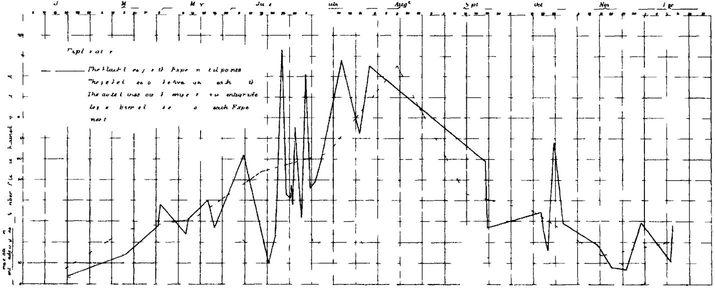
Table I—(Appendix Added March 5, 1887.)

Date. 1886.	Conditions of experiment.	Number of colonies obtained from given volume of air.	Number of colonies from 10 litres of air (calculated).	Number of micro-organisms falling on 1 horizontal square foot per minute.
November 13th. 11 A.M.—12.30 P.M.	Wind W. by N.W., *moderate, but rather variable in strength and direction.* Four days' previous rain. Sunshine, roads wet, pavement dry. 6·9° C.	39 in 20 lits 18 moulds.	20	88
November 20th. 11 A.M.	Wind S.W. by W., *gentle, but variable in strength and direction.* Roads and pavement still wet from rain on previous night. 12·5° C.	15 in 20 lits. 9 moulds.	8	43
November 26th. 2.45—3.35 P.M.	Wind E., *moderately strong, but variable in strength and direction.* Roads, &c., wet with dew. 8·6° C.	8 in 11 lits 7 moulds.	7	17
November 29th. 2.45 P.M.	Wind S.W., *very strong.* Roads wet, pavement dry. 9·7° C.	21 in 12 lits 8 moulds	17	157
December 3rd. 3 P.M.	Wind W. by N.W., *very gentle, but occasional gusts.* Everything dry with hard frost. 0·8° C.	29 in 10 lits 8 moulds.	29	125

Table I (Appendix)—*continued*

Date 1886	Conditions of experiment	Number of colonies obtained from given volume of air	Number of colonies from 10 litres of air (calculated)	Number of micro organism falling on 1 horizontal square foot per minute
December 16*th* 2—3 P M	Wind E by N E *gentle but variable in strength and direction* Roads and pavement wet very heavy rain two days before 5 0 C	15 in 13 lits 4 moulds	12	25 26
3—4 P M	Ditto Wind slightly more gentle	13 in 13 lits 4 moulds	10	39 31
December 17*th* 1.20 P M	Wind E *gentle* Foggy Roads &c wet 2 0° C	31 in 11 lits 12 moulds	28	74

AIR collected on the Roof of SCIENCE SCHOOLS SOUTH KENSINGTON MUSEUM 1886



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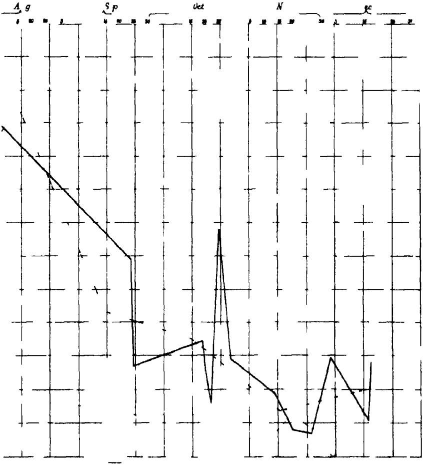

W Newman & Co th

April 21, 1887

Professor STOKES, D C L , President, in the Chair

The Presents received were laid on the table, and thanks ordered for them

The following Papers were read —

I "On Phosphonium Chloride" By SIDNEY SKINNER, B A , Scholar of Christ's College, Cambridge Communicated by Professor DEWAR, F R S Received March 28, 1887

At the close of an interesting paper in the 'Annales de Chimie,' vol 20, 1880, Ogier describes the preparation of the compound phosphonium chloride When the two gases phosphine and hydrochloric acid are mixed at 14° C under atmospheric pressure they do not combine, but when the pressure is raised to 20 atmospheres small crystals of the compound PH_4Cl form at the upper end of the tube At 26° C these crystals melt and the liquid formed was shown by some later experiments of van t Hoff ('Deutsch Chem Gesell Ber ,' Jahrg 18, p 2088) to reach the critical state about 50° This was the extent of our knowledge when the following experiments were begun with a view to examine more exactly this dissociable process, (1) in regard to its relations to temperature volume, and pressure, and to those of the separate gases PH_3 and HCl (2) in regard to the thermal changes involved in the formation of the compound

This paper deals with the first part of the research only, but owing to the interesting results which it contains there can be little hesitation in putting it forth

Experiments on Phosphonium Chloride.

Phosphine was prepared by dropping strong potash solution on a mixture of broken glass and phosphonium iodide After leaving the flask in which this operation was conducted the gas passed through a tube containing moist broken glass kept at the temperature of melting ice, and then through a drying tube of $CaCl_2$ and P_2O_5 The gas was collected in tubes over mercury The hydrochloric acid gas was prepared from strong sulphuric acid and ammonium chloride To obtain a mixture of equal volumes of these two gases, a pipette was constructed so that it could be filled to a certain mark under atmospheric pressure and at the temperature of the room. This being filled alternately with PH_3 and HCl, was discharged into a large glass tube. In this way I prepared a mixture of equal volumes of the two gases. Part of this was delivered into a Cailletet tube which was set up in one of the iron bottles of a Cailletet pump; in the other bottle was placed a well tested air gauge.

The temperature of the tube was maintained constant by a vapour jacket of acetone, and altered by varying the pressure under which the liquid boiled.

Observations were then taken for isothermals. The following, as useful for the present purpose, are extracted from those readings; the whole mass of the gas used being 18·0 c.c. at 0° and 760 mm.

Temp.	Saturated vol.	Pressure
7°	—	11·6 atmospheres.
13	0·84 c.c.	19·8 „
18	0·64 „	27·3 „
21	0·47 „	33·6 „
24	0·37 „	39·1 „
28·5	0·275 „	47·2 „
45	0·102 „	85·3 „

The critical point of the liquid was observed at 48° C. and under 95 atmospheres.

The temperature at which the crystals are formed under one atmosphere pressure is given as −30° by Ogier.

The formation of these crystals is evidently a dissociable one, for when the pressure is removed they decompose into equal volumes of the separate gases The question whether any chemical combination takes place before the appearance of crystals is one we shall discuss when considering the saturated volume curve of PH_4Cl in comparison with those of its constituents.

Experiments on Phosphine.

This gas, prepared and dried as before, was passed for some hours through a Cailletet tube from which the air had previously been displaced with carbonic acid gas. After careful sealing the tube was placed in the iron bottle, and liquid was readily obtained at the ordinary temperature under a pressure of about 30 atmospheres.

The critical state was reached with this liquid at 54° and 70·5 atmospheres pressures; so that it is easily observed when the tube is heated with acetone vapour. It is interesting to compare this critical point with that of the nitrogen analogue, ammonia, which does not reach the critical state till 130° and 115 atmospheres pressure. This is an unexpected result, as the substitution of a less volatile element has the effect of raising the critical temperature: thus for CO_2 critical temperature is 31·9° and for CS_2 278°.

I have also noticed the formation of a crystalline hydrate of phosphine when the gas is liquefied in the presence of water. The liquid gas floats on the surface of the water like benzene, and is apparently only slightly soluble. By suddenly increasing and decreasing the pressure the two liquids are mixed together, and after a short time a mass of crystals is formed.

The following table gives the results of measurements of the saturated volume, maximum pressure, and liquid volume at different temperatures.

Temperature.	Saturated volume.	Maximum pressure.	Liquid volume.	Density of the liquid.
	c.c.	atmospheres.	c.c.	
51°·4	..	..	0·160	0·402
49·4	0·362	62·4	0·154	0·417
44·4	0·468	56·1	0·137	0 469
39·4	0·558	50·8	0·128	0·502
29·4	0·744	41·3	0·120	0·536
24·6	0·851	37·1	0·118	0·545
18·4	..	32·6	0·115	0·559
8·4	..	27·2	0·108	0·595
2·4	..	23·4	0·104	0·618

The numbers in the column headed density are calculated by dividing the whole mass in grams of the gas by the volume of the liquid in cubic centimetres. The mass of the gas was 0·06435 gram.

The physical constants for hydrochloric acid gas used in the diagrams are calculated from Ansdell's paper ('Roy. Soc. Proc.,' vol. 30, 1880). The mass of the gas* he used was 0·08531 gram; and therefore the proper reduction has been made so that the volumes in the diagram may be those occupied by the molecular mass of hydrochloric acid in milligrams.

In the first diagram I have plotted for comparison the maximum pressure of the vapours at different temperatures both above and below zero. In the case of PH_4Cl the curve is drawn from my own observations from the critical point to 7°; that point is joined by a dotted curve to the point corresponding to 1 atmosphere at −30°, at which temperature Ogier states that the crystals are formed under atmospheric pressure. The curve for HCl below zero is from numbers given by Faraday ('Phil. Trans.,' 1846). The curve for PH_3 is from

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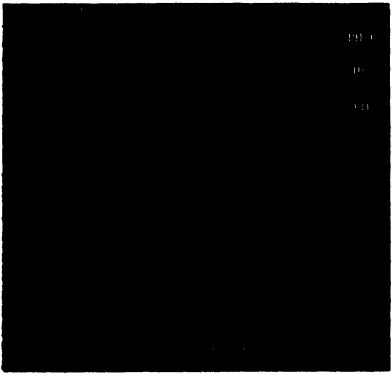
* Found by calculation from Ansdell's data.

Fig 1



my own observations, and would run alongside of that for HCl if continued below zero The boiling point of PH_3 is given by Olszewski as −85°

At first sight it is evident that the form of the curve for PH_4Cl is not a normal one for if it were it would lie in the same direction as the other two At all temperatures the pressure is much lower than that corresponding to a mixture of the two gases PH_3 and HCl, so that it is evident forces of chemical attraction are acting From −30° to 10° the curve runs normally, so it appears that the gas probably consists wholly, from reasons we shall presently point out, of PH_3 and HCl molecules Above 10° these two gases combine and the pressure necessary to deposit the crystals very rapidly increases If the temperature be maintained constant the whole of the mixture is converted to the crystals of the compound under constant pressure by reducing the volume, just as a saturated vapour is converted into a liquid under similar conditions

At the temperature at which the crystals melt there should be an alteration in the direction of the curve, if the phenomenon of fusion is analogous in this case to that of melting ice. However, such an alteration can scarcely be seen in the diagram, but would doubtless be made apparent by very careful observations.

FIG. 2.

In the second diagram I have plotted curves representing the saturated volumes at different temperatures. To make them comparable it is necessary to take definite masses of the gases. I have therefore plotted the volumes corresponding to 70·5 mgrms. PH_4Cl, 34 mgrms. PH_3, and 36·5 mgrms. HCl. It will be observed that about 10° the saturated volume of the mixture corresponds to the sum of that of its components, whilst about their critical points the volumes are approximately equal. This shows that at 10° we are dealing with a mixture of the two gases, whilst near the critical point the mixture has combined and the deposition of liquid takes place from gaseous PH_4Cl.

The volumes which the liquids occupy near their critical points are very nearly equal. This may be shown to be the case in another way. It has been proved from van der Waals' formula that the result of dividing the critical temperature in absolute degrees by the critical pressure gives a number proportional to the greatest volume a liquid can occupy (Dewar, 'Phil. Mag.,' vol. 18, 1884). Now in our case we have—

	T_c° C.	P_c.	$\frac{T_c \text{ abs.}}{P_c}$
		atmos.	
PH_4Cl............	48°	95	3 5
PH_3..............	54	70·5	4 6
HCl.............	52	86	3·7

If then 4·6 volumes of liquid phosphine near the critical temperature were brought in contact with 3·7 volumes of liquid HCl about the same temperature, combination would take place with condensation of volume to nearly one half. *This is a case of combination of liquids obeying a law very similar to Gay-Lussac's law* for the combination of gases under ordinary conditions of temperature and pressure. A symbolic representation of this combination would be :—

$$\begin{array}{ccccc} PH_3 & + & HCl & = & PH_4Cl. \\ \text{1 vol. liquid} & + & \text{1 vol. liquid} & = & \text{1 vol. liquid.} \end{array}$$

A close analogy evidently exists between the law of combination by volume of these liquids near their critical temperatures and Gay-Lussac's law; whether other cases of such a law of combination will be found is a question which cannot yet be answered.

Nevertheless the results in this paper appear to me to give answers to these two questions which Ogier states in his essay :—(1) Ce liquide est-il la combinaison même ou un simple mélange des gaz liquéfiés? C'est ce qu'il ne m'a pas été possible d'élucider; (2) Peut-être le chlorhydrate d'hydrogène phosphoré existe-t-il réellement à l'état gazeux à une température moins basse.

I hope to continue these experiments with a view to determine the heat of formation of this compound PH_4Cl, and also to determine the conditions of the formation and composition of the hydrate of phosphine.

I must express my best thanks to Professor Dewar, F.R.S., for his many kind and helpful suggestions, and also to the Master and Fellows of Christ's College, who have permitted me to retain my scholarship during the continuance of this work.

II. "On the Principal Electric Time-constant of a Circular Disk." By HORACE LAMB, M.A., F.R.S., Professor of Pure Mathematics in the Owens College, Victoria University. Received March 29, 1887.

The time-constant for currents of any normal type in a given conductor is the time in which free currents of that type fall to $1/e$ of their original strength. In strictness there are for any conductor an infinite series of time-constants, corresponding to the various normal types, but in such a case as that of a coil of wire one of these is very great in comparison with the rest, which belong to types in which the current is in opposite directions in different parts of a section of the wire. And in all cases the time-constant corresponding to the most persistent type which can be present under given circumstances is, of course, the one which is most important from an experimental point of view.

A determination of the time-constants of a uniform circular disk would be of interest for two reasons: first, in relation to Arago's rotations, which are entirely due to the greater or less persistence of currents once started in the disk; and, secondly, in connexion with Professor Hughes's experiments with the induction balance, in which the disturbance produced in the field by the currents induced in metal disks (such as coins) was studied. Unfortunately, the mathematical problem thus suggested would seem to be difficult. Restricting ourselves, for simplicity, to cases where the currents flow in circles concentric with the disk, so that the problem is not complicated by the existence of an electric potential, then if ϕ be the current-function, the electric momentum at a distance r from the centre of the disk will be $-dP/dr$, where P is the potential of an imaginary distribution of matter of density ϕ over the disk. Hence, if ρ' be the resistance per unit area, we have—

$$\rho'\frac{d\phi}{dr} = -\frac{d}{dt}\frac{dP}{dr}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

In any normal type, ϕ and P will vary as $e^{-\lambda t}$, and, therefore—

$$\rho'\frac{d\phi}{dr} = \lambda\frac{dP}{dr}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

or, in the case of uniform resistance—

$$\rho'\phi = \lambda\dot{P}+C$$

over the disk.

In the absence of a rigorous solution of this problem (which seems well worthy the attention of mathematicians), a good approximation to the principal time-constant may be obtained on the following principles:—*

1. An increase of resistance in any part of the disk will diminish the time-constant; and

2. If the time-constant be calculated on any arbitrary assumption as to the distribution of current, the result will be an *under*-estimate, and will, moreover, be a close approximation to the true value if the assumed law be not very wide of the mark, on account of the "stationary" property of the normal types.

Some distributions of density ϕ, and corresponding potentials P, convenient for our purpose, are obtained by considering the disk as a limiting form of heterogeneous ellipsoid, in which the surfaces of equal density are similar and coaxial ellipsoids.† If the density at any point Q in the interior of the ellipsoid—

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2} = 1$$

be

$$C(1-\theta^2)^n,$$

where θa, θb, θc are the semi-axes of the similar ellipsoid through Q, the corresponding potential at internal points will be—

$$C\cdot\frac{\pi abc}{n+1}\int_0^\infty\left(1-\frac{x^2}{a^2+k}-\frac{y^2}{b^2+k}-\frac{z^2}{c^2+k}\right)^{n+1}\frac{dk}{\sqrt{\{(a^2+k)(b^2+k)(c^2+k)\}}}.$$

Putting $a=b$, and passing to the case of a disk, by putting $c=0$, $2Cc=1$, we find that to the surface-density—

$$\phi = \left(1-\frac{r^2}{a^2}\right)^{n+\frac{1}{2}}\int_0^{\frac{1}{2}\pi}\cos^{2n+1}\chi\,d\chi = \frac{\pi^{\frac{1}{2}}}{2}\cdot\frac{\Gamma(n+1)}{\Gamma(n+\frac{3}{2})}\left(1-\frac{r^2}{a^2}\right)^{n+\frac{1}{2}}, \quad (3.)‡$$

corresponds, for points of the disk, the potential—

$$P = \frac{\pi a^2}{2(n+1)}\int_0^\infty\left(1-\frac{r^2}{a^2+k}\right)^{n+1}\frac{dk}{(a^2+k)k^{\frac{1}{2}}}$$

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* See Rayleigh's 'Sound,' §§ 88, 305, &c.

† See Ferrers, 'Quart. Journ. Math.,' vol. 14, p. 1.

‡ In the electrical application we must suppose $n+\frac{1}{2}>0$.

$$= \frac{\pi a}{n+1}\int_0^{\pi}\left(1-\frac{r^2}{a^2}\sin^2\chi\right)^{n+1}d\chi$$

$$= \frac{\pi^2 a}{2(n+1)}\,\mathrm{F}\left(-n-1,\ \tfrac{1}{2},\ 1,\ \frac{r^2}{a^2}\right), \quad . \quad . \quad . \quad . \quad (4.)$$

in the usual notation of hypergeometric series.

In the electrical problem, then, to a current-function of the form (3) corresponds the current-strength—

$$-\frac{d\phi}{dr} = \frac{\sqrt{\pi}}{a}\cdot\frac{\Gamma(n+1)}{\Gamma(n+\frac{1}{2})}\frac{r}{a}\left(1-\frac{r^2}{a^2}\right)^{n-\frac{1}{2}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

and the electric momentum—

$$-\frac{d\mathrm{P}}{dr} = \frac{\pi^2}{2}\cdot\frac{r}{a}\,\mathrm{F}\left(-n,\ \tfrac{3}{2},\ 2,\ \frac{r^2}{a^2}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

The assumption (3) will correspond accurately to a normal type for a non-uniform disk, provided the law of resistance be properly adjusted. For (2) is satisfied if—

$$\frac{\rho'}{a}\frac{\Gamma(n+1)}{\Gamma(n+\frac{1}{2})}\left(1-\frac{r^2}{a^2}\right)^{n-\frac{1}{2}} = 2\lambda\pi^{\frac{1}{2}}\int_0^{\pi}\left(1-\frac{r^2}{a^2}\sin^2\chi\right)^{n}\sin^2\chi\,d\chi$$

$$= \lambda\frac{\pi^{\frac{1}{2}}}{2}\,\mathrm{F}\left(-n,\ \tfrac{3}{2},\ 2,\ \frac{r^2}{a^2}\right),$$

that is, $$\rho' = \frac{4}{\pi}\rho_0'\int_0^{\frac{1}{2}\pi}\left(1-\frac{r^2}{a^2}\sin^2\chi\right)^{n}\sin^2\chi\,d\chi \div \left(1-\frac{r^2}{a^2}\right)^{n-\frac{1}{2}} \quad . \quad (7.)$$

$$= \rho_0'\mathrm{F}\left(-n,\ \tfrac{3}{2},\ 2,\ \frac{r^2}{a^2}\right) \div \left(1-\frac{r^2}{a^2}\right)^{n-\frac{1}{2}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

where ρ_0' is the resistance at the centre; and the time-constant is—

$$\tau = \lambda^{-1} = \frac{\Gamma(n+\frac{1}{2})}{\Gamma(n+1)}\,\frac{\pi^{\frac{1}{2}}a}{2\rho_0'}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

For $n = 0$, we have—

$$\rho' = \rho_0'(1-r^2/a^2)^{\frac{1}{2}},$$

$$\tau = \frac{\pi^2 a}{2\rho_0'}.$$

Since ρ' diminishes from the centre outwards, we see that $4{\cdot}935a/\rho'$ is a superior limit for a disk of uniform resistance ρ'.

For $n = \frac{1}{2}$,

$$\rho' = \frac{4}{\pi}\rho_0' \int_0^{\frac{1}{2}\pi} \left(1 - \frac{r^2}{a^2}\sin^2\chi\right)^{\frac{1}{2}} \sin^2\chi \, d\chi,$$

$$\tau = \frac{\pi a}{\rho_0'},$$

from which it appears that $3{\cdot}142 a/\rho'$ is a superior limit for a uniform disk.

For $n = 1$,

$$\rho' = \rho_0' \left(1 - \tfrac{3}{4}\frac{r^2}{a^2}\right) \div \left(1 - \frac{r^2}{a^2}\right)^{\frac{1}{2}}, \quad \ldots\ldots \quad (10.)$$

$$\tau = \frac{\pi^2 a}{4\rho_0'}. \quad \ldots\ldots\ldots\ldots \quad (11.)$$

This case is remarkable as giving a resistance nearly uniform over the disk, except close to the edge, where it rapidly increases. The second column in the following table gives the resistance at various distances from the centre; the third column, the corresponding thicknesses in terms of the thickness at the centre, the material of the disk being supposed uniform.

r/a.	ρ'/ρ'_0.	Thickness.
0	1·000	1·000
0·1	0·998	1·002
0·2	0·990	1·010
0·3	0·978	1·023
0·4	0·960	1·041
0·5	0·938	1·066
0·6	0·913	1·096
0·7	0·886	1·129
0·8	0·867	1·154
0·9	0·900	1·111
0·95	1·085	0·966
0·99	1·878	0·532
1·00	∞	0

The minimum value of ρ' is $0{\cdot}8660\rho_0'$, corresponding to $r/a = 0{\cdot}8165$. Denoting this minimum by ρ_1', we find from (11)—

$$\tau = 2{\cdot}137 \frac{a}{\rho_1'}.$$

This is an *inferior* limit to the value of τ for a disk of uniform resistance ρ_1'.

Some further information may be gathered from the second principle stated above. The electrokinetic energy of the system of currents defined by (3) is—

$$T = \tfrac{1}{2}\int_0^a \frac{d\phi}{dr}\frac{dP}{dr} \,.\, 2\pi r\,dr$$

$$= \frac{\pi^{\frac{1}{2}}a}{4}\frac{\Gamma(n+1)}{\Gamma(n+\frac{1}{2})}\int_0^1 z(1-z)^{n-\frac{1}{2}}\,F(-n,\,\tfrac{3}{2},\,2,\,z)\,dz.$$

The integral

$$= \Sigma_m \,.\, \frac{\Gamma(-n+m)\,.\,\Gamma(\frac{3}{2}+m)}{\Gamma(m+1)\Gamma(-n)\Gamma(\frac{3}{2})}\cdot\frac{\Gamma(2)}{\Gamma(2+m)}\int_0^1 z^{m+1}(1-z)^{n-\frac{1}{2}}dz$$

$$= \Gamma(n+\tfrac{1}{2})\,.\,\Sigma_m \frac{\Gamma(-n+m)\Gamma(\frac{3}{2}+m)}{\Gamma(m+1)\Gamma(-n)\Gamma(\frac{3}{2})}\,\frac{1}{\Gamma(m+n+\frac{5}{2})}$$

$$= \frac{\Gamma(n+\frac{1}{2})}{\Gamma(n+\frac{5}{2})}F_1(-n,\,\tfrac{3}{2},\,n+\tfrac{5}{2})$$

$$= \frac{\Gamma(n+\frac{1}{2})\Gamma(2n+1)}{\Gamma(2n+\frac{5}{2})\Gamma(n+1)}.$$

Hence
$$T = \frac{\pi^{\frac{1}{2}}a}{4}\cdot\frac{\Gamma(2n+1)}{\Gamma(2n+\frac{5}{2})}. \quad . \quad . \quad . \quad . \qquad (12.)$$

If the disk be of uniform resistance ρ', the dissipation is—

$$W = \rho'\int_0^a \left(\frac{d\phi}{dr}\right)^2 .\, 2\pi r\,dr$$

$$= \pi^2\rho' \,.\, \left\{\frac{\Gamma(n+1)}{\Gamma(n+\frac{1}{2})}\right\}^2\int_0^1 z(1-z)^{2n-1}dz$$

$$= \frac{\pi^2\rho'}{2n(2n+1)}\left\{\frac{\Gamma(n+1)}{\Gamma(n+\frac{1}{2})}\right\}^2. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13)$$

Introducing a time-factor $e^{-t/\tau}$, and supposing that the system of currents is constrained to remain of the type (3) during the decay, we find on equating the rate of diminution of the energy to the dissipation—

$$\tau = \frac{\pi^{\frac{1}{2}}a}{\rho'}\frac{\Gamma(2n+2)\{\Gamma(n+\frac{1}{2})\}^2}{\Gamma(n)\Gamma(n+1)\Gamma(2n+\frac{5}{2})}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14.)$$

Any value of τ obtained from this formula will be an inferior limit to the true value. The following table gives the values of $\tau\rho'/a$ for different values of n:—

n	$\tau\rho'/a$.
0·5	2·133
0·6	2·201
0·7	2·239
0·8	2·257
0·9	2·261
1·0	2·256
1·1	2·245
1·2	2·229
1·3	2·210
1·4	2·189
1·5	2·167
2·0	2·051

It appears that the value (14) of τ is a maximum for $n = 0·9$ about, and, hence, that the principal time-constant of a circular disk is not less than $2·26a/\rho'$. We have seen that the value of ϕ obtained by putting $n = 1$ in (3) must be a pretty fair representation of the most persistent type of free currents in a uniform disk, and the case of $n = 0·9$ will not be materially different. The "stationary" property already alluded to therefore warrants us in asserting that the value just given must be a close approximation to the truth. If δ be the thickness, ρ the specific resistance of the material, we may write our result thus—

$$\tau = 2·26\,\frac{a\delta}{\rho}.$$

For a disk of copper [$\rho = 1600$ C.G.S.], a decimetre in radius, and 2·5 mm. in thickness, this gives $\tau = 0·0035$ sec.

Addendum.—April 11, 1887.

In the above calculations it is assumed that the current-intensity is sensibly uniform throughout the thickness of the disk. This will be the case, at all events for a non-magnetisable substance, if the radius be a moderately large multiple of the thickness. To examine this point more closely, it will be sufficient to consider a simpler problem in which all the circumstances can be calculated with exactness. Let us suppose then that we have a system of free currents everywhere parallel to the axis of z in a stratum of conducting matter bounded by the planes $y = \pm\delta/2$. With the usual notation we shall have—

$$F = 0, \qquad G = 0,$$

$$a = \frac{dH}{dy}, \qquad b = -\frac{dH}{dx}, \qquad c = 0.$$

In the spaces on each side of the stratum—

$$\frac{d^2H}{dx^2} + \frac{d^2H}{dy^2} = 0;$$

whilst in the conductor itself—

$$\frac{d^2H}{dx^2} + \frac{d^2H}{dy^2} = -4\pi\mu w.$$

The equation of electromotive force is—

$$\rho w = -\frac{dH}{dt} = \lambda H,$$

the time-factor, as before, being $e^{-\lambda t}$. Let us further assume that x enters into the value of H only through a factor $\sin mx$. We shall then have, in the conductor—

$$\frac{d^2H}{dy^2} + k^2 H = 0,$$

where $$k^2 = 4\pi\mu\lambda/\rho - m^2, \quad \ldots\ldots \quad (15.)$$

and in the external spaces—

$$\frac{d^2H}{dy^2} - m^2 H = 0.$$

The solutions of these equations, appropriate to our present problem, are

$$H = D \cos ky,$$

and $$H = D' e^{\mp my},$$

respectively, the upper or lower sign being taken in the latter expression according as y is positive or negative. At the surfaces of the conductor we must have—

$$a = \mu a', \qquad b = b',$$

when the accented letters relate to points just outside, the unaccented to points just inside. These conditions give—

$$k\mathrm{D} \,.\, \sin\frac{k\delta}{2} = \mu m \mathrm{D}' e^{-m\delta/2} \cos\frac{k\delta}{2},$$

$$\mathrm{D} = \mathrm{D}' e^{-m\delta/2},$$

whence $$k\delta \,.\, \tan\frac{k\delta}{2} = \mu m\delta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (16.)$$

If $\mu = 1$, as we have supposed, and $m\delta$ is small, the principal root of this equation in $k\delta$ is small, and the current-intensity, which varies as $\cos ky$, will be nearly uniform throughout the thickness. The equation (16) then gives—

$$k^2\delta^2 = 2m\delta - \tfrac{1}{3}m^2\delta^2 + \&c.,$$

and therefore from (15)—

$$\tau = \lambda^{-1} = \frac{2\pi}{m\rho'}\left(1 - \tfrac{1}{3}m\delta + \&c.\right),$$

where $\rho' = \rho/\delta$. For the purpose of a rough comparison with our original problem we may suppose that π/m is comparable with R, the radius of the disk. It follows that the effect of replacing the actual disk, of finite thickness, by an infinitely thin disk of the same conductivity (per unit area) is to increase the time-constant by the fraction δ/R of itself, about.

In an *iron* plate, on the other hand, the current-intensity will fall off considerably from the median plane to the surface, unless the ratio δ/R be extremely small. For instance, if $\mu m\delta = \pi/2$, or say $\delta/\mathrm{R} = 1/2\mu$, the principal root of (16) is $k\delta = \pi/2$, and the intensity at the surface is only 0·71 of its value in the median plane, although the thickness of the disk may perhaps not exceed one-thousandth of the radius. Again if, $m\delta$ being still small, $\mu m\delta$ is moderately large, we shall have $k\delta = \pi$, nearly, so that the current-intensity almost vanishes at the surface. In such a case—

$$\tau = 4\pi\mu/k^2\rho = 4\mu\delta^2/\pi\rho,$$

roughly. It will be seen that within certain limits (*e.g.*, if $\mu = 500$ and the lateral dimensions be not more than about 100 times the thickness) this result is independent of the size and shape of the plate. Under these circumstances, the value of τ for an iron plate ($\rho = 10{,}000$ C.G.S.) whose thickness is 2·5 mm. will be comparable with 0·003 sec.

III. "On Parts of the Skeleton of *Meiolania platyceps* (Ow.)." By Sir RICHARD OWEN, K.C.B., F.R.S., &c. Received March 29, 1887.

(Abstract).

The subjects of the present paper are additional fossil remains of *Meiolania platyceps* from Lord Howe's Island, transmitted to the British Museum since the author's previous paper on the subject. Additional cranial characters are defined and illustrated by drawings of more or less perfect specimens of the skull, of vertebræ of the neck, trunk, and tail, of limb-bones, and portions of the dermal skeleton.

The author sums up the affinities, deducible from the above parts of the skeleton, to the orders *Chelonia* and *Sauria*, with grounds for the conclusion that the genera *Megalania* and *Meiolania* are more nearly akin to the Saurian division of the class *Reptilia*, in which he proposes to refer those extinct genera to a sub-order called *Ceratosauria*.

IV. "Some Applications of Dynamical Principles to Physical Phenomena. Part II." By J. J. THOMSON, M.A., F.R.S., Fellow of Trinity College and Cavendish Professor of Experimental Physics in the University of Cambridge. Received March 31, 1887.

(Abstract.)

This is a continuation of a paper with the same title published in the 'Phil. Trans.,' 1885, Part II. In the first paper dynamical principles were applied to the subjects of electricity and magnetism, elasticity and heat, to establish relations between phenomena in these branches of physics. In this paper corresponding principles are applied to chemical and quasi-chemical processes such as evaporation, liquefaction, dissociation, chemical combination, and the like.

Many of the results obtained in this paper have been or can be obtained by means of the Second Law of Thermodynamics, but one of the objects of the paper is to show that there are other ways of attacking such questions, and that in many cases such problems can be solved as readily by the direct use of dynamical principles as by the Second Law of Thermodynamics.

A great deal has been written on the connexion between the Second Law of Thermodynamics and the principle of Least Action; some of these investigations are criticised in the first part of the

paper. After this it is shown that for a collection of molecules in a steady state, the equation (which for ordinary dynamical systems is identical with the well-known Hamiltonian principle)

$$\delta(\bar{T}-\bar{V}) = 0$$

is satisfied, where $\bar{T}$ and $\bar{V}$ are respectively the mean values of the kinetic and potential energies taken over unit time, and where the variation denoted by δ is of the following kind.

The coordinates fixing the configuration of any physical system, consisting according to the molecular theory of the constitution of bodies of an immense number of molecules, may be divided into two classes:—

(*a*.) Coordinates, which we may call molar, which fix the configuration of the system as a whole; and

(*b*.) Molecular coordinates which fix the configuration of individual molecules.

We have the power of changing the molar coordinates at our pleasure, but we have no control over the molecular coordinates.

In the equation—

$$\delta(\bar{T}-\bar{V}) = 0$$

only the molar coordinates are supposed to vary, all velocities remaining unchanged. Hence in applying this equation we need only consider those terms in $\bar{T}$ and $\bar{V}$ which involve the molar coordinates, and expressions for these terms for gases, liquids, and solids are given in the paper; the rest of the paper after these have been obtained consists of applications of the above equation.

The density of a vapour in equilibrium with its own liquid is obtained as a function of the temperature, and the effect upon the density of such things as the curvature or electrification of the surface of the liquid is determined.

The phenomenon of dissociation is next investigated, and an expression for the density of a dissociated gas obtained which agrees substantially in form with that given by Professor Willard Gibbs in his well-known paper on the "Equilibrium of Heterogeneous Substances."

The effect of pressure upon the melting point of solids and the phenomena of liquefaction are then investigated, and the results obtained for the effect of pressure upon the solubility of salts are shown to agree with the results of Sorby's experiments on this subject. The effect of capillarity upon solubility is investigated, and it is shown that if the surface-tension increases as the salt dissolves then capillarity tends to diminish the solubility, and *vice versâ*.

The question of chemical combination is then considered, particularly the results of what is called by the chemists "mass-action," of which a particular case is the division of a base between two acids.

The general problem investigated is that in which we have four substances A, B, C, D present, such that A by its action on B produces C and D, while C by its action on D produces A and B. The relation between the quantities of A, B, C, D present when there is equilibrium is obtained and found to involve the temperature; when the temperature is constant it agrees in some cases with that given by Guldberg and Waage, though in others it differs in some important respects. Thus if ξ, η, ζ, ϵ be the number of molecules of A, B, C, D respectively, when there is equilibrium, θ the absolute temperature, H the amount of heat given out when the chemical process which results in the increase of ξ by unity takes place, and k a quantity which is the same for all substances, then it is proved that—

$$\frac{\xi^p \eta^q}{\zeta^r \epsilon^s} = \mathrm{C}e^{\frac{p\mathrm{H}}{k\theta}},$$

where C is a constant; p, q, r, s are quantities such that if (A) represents the molecule of A, with a similar notation for the other molecules, then the chemical reaction can be represented by the equation—

$$p\{\mathrm{A}\}+q\{\mathrm{B}\} = r\{\mathrm{C}\}+s\{\mathrm{D}\}.$$

Thus if A, B, C, D be respectively sulphuric acid, sodium nitrate, nitric acid, and sodium sulphate, in which case the reaction is represented by—

$$\mathrm{H_2SO_4+2NaNO_3 = 2HNO_3+Na_2SO_4}.$$

Then if the molecules of sodium nitrate and nitric acid be represented by $NaNO_3$ and HNO_3,

$$p = 1, \quad q = 2, \quad r = 3, \quad \text{and } s = 1.$$

If, however, the molecules of sodium nitrate and nitric acid are represented respectively by $Na_2N_2O_6$ and $H_2N_2O_6$, then since the chemical reaction may be written—

$$\mathrm{H_2SO_4+Na_2N_2O_6 = H_2N_2O_6+Na_2SO_4},$$

$$p = 1, \quad q = 1, \quad r = 1, \quad \text{and } s = 1.$$

According to Guldberg and Waage the relation between ξ, η, ζ, ϵ is—

$$\xi\eta = k\zeta\epsilon;$$

this when the temperature is constant, agrees with the above expression if $p = q = r = s$.

We see that the state of equilibrium will vary rapidly with the temperature if H be large, that is, if the chemical process is attended by the evolution of a large quantity of heat.

The effect of alterations in the external circumstances such as those which may be produced by capillarity, pressure, or electrification are investigated, and it is shown that anything giving rise to potential energy which increases as the chemical combination goes on tends to stop the combination.

The last part of the paper is taken up with the consideration of irreversible effects such as those accompanying the passage of electric currents through metallic conductors or electrolytes. These are looked upon as the average of a large number of discontinuous phenomena which succeed each other with great rapidity. The ordinary electrical equations with the usual resistance terms in, represent on this view the average state of the system, but give no direct information about its state at any particular instant. It is shown that if we take this view we can apply dynamical principles to these irreversible effects, and the results of this application to the case of electrical resistance are given in the paper.

V. "Conduction of Heat in Liquids." By C. CHREE, B.A., King's College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received March 31, 1887.

(Abstract.)

In this research the liquid layer through which the conduction takes place is of a moderate thickness, the object being to obtain results not open to the objections which can be raised against most previous methods, in which conduction has taken place through layers of very small thickness.

Two similar forms of apparatus, differing chiefly in size, were employed, but from the larger apparatus few results were obtained, and to those little independent weight is assigned.

The liquid was contained in a wooden tub, and heat was applied by pouring hot water into a metal dish supported so as to be in contact with the liquid surface. At a given depth was fixed a fine platinum wire, and the variation in its temperature was determined by observing the variation in its electrical resistance. By this means the temperature at a given depth in the liquid is determined for any instant subsequent to the application of heat.

In applying the heat a given quantity of water, heated to a given temperature, was suddenly poured into the metal dish, and the time noted. In one set of experiments this water was after a given interval siphoned from the dish, in another set it was left undisturbed. In either case the variation in the temperature of the platinum wire, as indicated by the change in its resistance, was determined by observations of the readings of a delicate galvanometer, which was affected by the variation in an electrical current traversing the platinum wire. The galvanometer readings supplied data from which could be calculated the interval that elapsed after the application of heat before the temperature in the liquid surrounding the platinum wire was rising fastest. An independent series of experiments gave the rate at which heat passed into each liquid from the dish.

To calculate the conductivity a mathematical investigation is carried out, which leads to an equation connecting the conductivity, density, and specific heat of a liquid with the time elapsed after the application of heat at the surface before the heating at a given depth should be most rapid. Though this equation cannot be directly solved, solutions of a close degree of approximation can be obtained. The density and specific heat being known, these solutions enable the conductivity to be calculated in absolute measure.

The liquids examined were water, paraffin and turpentine oils, bisulphide of carbon, methylated spirit, and solutions of various strengths of sulphuric acid and water. In the case of paraffin oil, methylated spirit, and water, the two different methods were employed, and the results agreed fairly well. In the case of turpentine the water was never siphoned, and in the case of the remaining liquids the siphon was always used. It was found that the conductivity of the various sulphuric acid solutions, some of considerable strength, differed very slightly from that of water, and thus there is a marked distinction between conducting powers for heat and for electricity. The presence of small impurities in the liquids, such as small quantities of salt, had no appreciable effect on the conductivity.

The intervals that elapsed after the application of heat before the temperature at the given depth was rising fastest did not differ very largely for the various liquids. It was shortest for the bisulphide and longest for turpentine. Owing to the comparatively small variation in this interval the value of the conductivity depends largely on the product of the density into the specific heat, a quantity to which it is directly proportional.

The values actually obtained are the following:—In those under column 1 the water was siphoned from the dish, in column 2 it was not. The units are centimetre and minute.

Liquid.		Column 1	Column 2
Water		0 0747	0 0815
Solution sulphuric acid	No 1	0 0759	—
"	No 2	0 0767	—
	No 3	0 0765	—
	No 4	0 0778	—
Methylated spirit		0 0354	0 0346
Bisulphide of carbon		0 03[illegible]2	—
Paraffin oil		0 0264	0 0273
Turpentine oil		—	0 0189

The temperature of the various experiments differed somewhat but as a rule was a little under 20° C The difference of temperature in the two series of experiments on water tends partly to explain the discrepancy in the above results as the results of previous observers indicate a considerable rise in conductivity with the temperature For water and the methylated spirit results of a confirmatory nature were obtained by the larger apparatus

The experiments were conducted in the Cavendish Laboratory

Presents April 21 1887

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Buckhurst Hill —Essex Field Club The Essex Naturalist No 3 8vo *Buckhurst Hill* 1887 The Club

London —London Mathematical Society Proceedings Nos 275–282 8vo *London* 1887 The Society

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K. K. Geologische Reichsanstalt. Abhandlungen. Band XII. Nr. 4. 4to. *Wien* 1886; Jahrbuch. Band XXXVI. Heft 4. 8vo. *Wien* 1886; Verhandlungen. Jahrg. 1886. Nr. 13-18. Jahrg. 1887. Nr. 1. 8vo. *Wien.* The Institute.

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Kosmos, an Eclectic Monthly Journal. Vol. I. Nos. 1-2. 4to *San Francisco* 1887. The Editor.

Naturalist (The). No. 141. 8vo. *London* 1887. The Editors.

Revista do Observatorio. Anno II. Num. 3. 8vo. *Rio de Janeiro* 1887. The Observatory.

Scientific News. Vol. I. No. 2. 4to. *London* 1887. The Editor.

April 28, 1887.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

1. "Note on Dr. G. J. Hinde's Paper 'On Beds of Sponge-remains in the Lower and Upper Greensand of the South of England' ('Philosophical Transactions,' 1885, p. 403)." By EDWARD HULL, LL.D., F.R.S., &c., Director of the Geological Survey of Ireland. Received March 17, 1887.

In a valuable communication read before the Society in May, 1885, Dr. Hinde has given an account of the bands of siliceous material, generally in the form of "chert," found at intervals in the two Greensand formations of the Cretaceous period throughout the south of England—clearly indicating the extent to which siliceous sponges contributed to the formation of the successive sea-beds of this period; an extent to which, as the "Challenger" soundings show, has its parallel in some parts of the ocean at the present day.

In discussing the origin of the chert and chalcedonic bands in which the spicules are imbedded, or out of which they have been dissolved, leaving cavities in their place, Dr. Hinde states his opinion that "There can scarcely be room for doubting that the beds and irregular masses of chert . . . have been derived from the silica of these sponge-remains; and *from the same source has also originated the silica** which, in many of the deposits—more particularly in the Blackdown Hills—has replaced the shells and tests of the mollusca and other calcareous organisms." He proceeds to say, "The theory has, however, been advocated that the silica of the chert has been derived rather as a direct deposit of this mineral from solution in sea-water, than as the product of the decomposition of the siliceous structure of the sponges. Thus Dr. Bowerbank held that the sponges imbedded in the chert of the Greensand possessed horny and not siliceous skeletons, and that the silica in the chert in which they were imbedded was attracted from the exterior medium by the animal matter, and not secreted from the living sponge. Professor T. Rupert Jones maintains the view that the silica of the chert is derived directly from sea-water; and similar opinions as to the origin of the chert bands in the Upper Carboniferous limestones of Ireland have been put forward by Messrs. Hull and Hardman, and by M. Renard,* with respect to the *phthanites* in rocks of the same age in Belgium. It is a significant fact, however, in connexion with the chert-beds of the Irish Upper Carboniferous strata that some have been discovered filled with sponge-spicules like the chert of the English Greensand, and this indicates a similar origin for the silica, and negatives the supposition of Professor Hull that it was deposited "from warm shallow water charged with silica in solution, in which chemical reactions would be at once set up, favoured and promoted by tidal and other currents."†

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* The italics are not in the original.

I have taken pains to quote the entire passage in Dr. Hinde's paper in order to avoid the possibility of misrepresentation; and I must confess my inability to understand the reasoning of the author. He regards the sponge-spicules as "the source" of the silica, and by their decomposition in the presence of sea-water as having given origin to the beds of chert; but the question arises, from what source did the sponge-skeletons themselves derive the silica from which they were formed? This could not have been from repeated solution and reconstruction, because by this process the supply of silica would have been used up. The statement, therefore, that "the beds and irregular masses of chert have been derived solely from the silica of the sponge-remains instead of from that held in solution by the sea-waters themselves" is altogether unintelligible. The real "source" of the silica is that small amount of this mineral which is always present in ocean waters; from this source the sponge-structures have been derived by organic agency, and without that agency the silica would seldom be solidified. Sponge siliceous skeletons are in reality the result of the presence of silica in sea-water—not its cause. If there had been no soluble silica there had been no siliceous sponges. But I am only here concerned with a defence of the views arrived at, after full investigation by my colleague, Mr. Hardman, and myself,* and corroborated by the independent investigations of M. Renard in Belgium;† and I wish to show how improbable it is that siliceous sponges could, by their dissolution, have taken any important part in the formation of the chert-beds of the Carboniferous Limestone either of Ireland or Belgium, or as far as I am aware of any other country. My argument will be based on the fact that the development of sponge-life in the seas of the Carboniferous period was insignificant, and quite inadequate to account for the existence of bands and masses of chert, sometimes constituting almost a half or a third of the entire mass of the Upper Limestone.‡

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* Dr. Hinde, in referring to Prof. Renard's Memoir ('Bulletin de l'Académie Royale de Belgique,' vol. 46, 1878, p. 471), goes so far as to question the author's determination of the nature of the "circular sections" shown in one of the figures (fig 2) accompanying the paper. The author identifies them as crinoid stems, Dr. Hinde suggests that they are really sponge-spicules; a view that no one so well acquainted with the Carboniferous Limestone as Professor Renard will for a moment admit.

† Dr. Hinde does not mention his authority for the statement of the abundance of sponge-spicules in the Carboniferous Limestone of Ireland, and I suspect that he has in this case, as in that of the phthanites of Belgium, mistaken the sections of crinoids for those of sponge-structures. There is no doubt some difficulty in distinguishing sections of sponge-spicules from ill-preserved segments of crinoid stems such as occur in chert; so that their identity must be determined by the forms which are prevalent in the ordinary limestones.

Let us now enquire what are the relative proportions of the genera and species of siliceous sponge-structures to those of calcareous forms both in Carboniferous and Cretaceous strata—assuming that the genera and species indicate to some extent the numerical development of these respective forms. In this comparison I shall omit from consideration the mollusca and molluscoidea—though in themselves very important, and altogether lime-forming organisms. In drawing up the following table (p. 307) I have availed myself of the lists published recently by Mr. Etheridge, F.R.S., which make the comparison simple and easy.§

The contrast of the non-molluscan fauna of the two periods will be at once apparent (1) in the enormous proportion of siliceous sponges in the Cretaceous as compared with those of the Carboniferous periods; and (2) in the predominance of corals and crinoids in the Carboniferous period. The insignificant representation of siliceous sponge-structures in the Carboniferous seas as compared with the calcareous foraminifera, corals, and crinoids will also be apparent. As compared with the development of these forms in the Carboniferous period, it will be seen that the species of siliceous sponges might almost be counted on the fingers of the two hands; both in genera, species, and individuals they are quite unimportant as compared with the calcareous organisms of that period, and totally inadequate to supply material for the formation of such beds of chert as are formed in the Carboniferous Limestone formation. The enormous predominance of the calcareous organisms in this formation is a fact which cannot be

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* "On the Nature and Origin of Beds of Chert of the Upper Carboniferous Limestone of Ireland." 'Scientific Transactions of the Royal Dublin Society,' vol. 1, 1878.

† "Recherches lithologiques sur les phthanites du calcaire carbonifère de Belgique." Par M. A. Renard. 'Bulletin de l'Académie Royale de Belgique,' vol. 46, 1878.

‡ As in the case of the Upper Limestone of Florence Court, near Enniskillen, altogether 400 feet thick, of which perhaps 150 are formed of chert-bands, intercalated with those of limestone.

§ Phillips' 'Manual of Geology,' Edit. 1885, Part II.

Table showing the Genera and Species of Invertebrata, other than Mollusca, in the Carboniferous and Cretaceous Periods.

	Carboniferous		Cretaceous.		Observations.
	Genera	Species	Genera.	Species	
Protozoa { Siliceous sponges	6	12	74	162	There is a slight uncertainty regarding the numerical proportion of the siliceous and calcareous sponges of the Carboniferous period
Protozoa { Calcareous ,,	2	2	13	50	
Foraminifera	15	43	39	171	
Hydrozoa	2	3	?	?	
Actinozoa	39	144	37	76	
Echinodermata { Echini, &c.	9	30	43	188	
Echinodermata { Crinoids ..	18	109	5	13	
Annulosa	11	34	4	14	
Crustacea	28	137*	40	110	
Polyzoa..................	..	..	59	114	
Total { Siliceous sponges	6	12	74	162	
Total { Calcareous organisms	121	514	240	736	

* Chiefly Entomostraca of the Upper Carboniferous stage.

disputed by those who have had opportunities of studying its characters, either in the north of England, in Ireland, or in Belgium, where whole beds may be observed composed almost entirely of crinoid stems and corals, while the microscope generally reveals other calcareous forms, such as those of foraminifera, which are invisible to the naked eye, or under the lens. If, then, these original calcareous structures have become silicified, whence could the silica have been derived if not from the circumambient waters of the ocean under certain special and favourable conditions of temperature?

In his paper on the origin of the beds and nodules of chert (phthanite) in the Carboniferous limestone of Belgium, M. Renard expressly identifies crinoid structures, not only in circular disks of the cross-section of the stems or ossicles, but in the more solid and structureless masses of the chert when treated with acid;* and he expressly states that the silicification has supervened in the case of an originally calcareous rock-compound chiefly of foraminifera, crinoids, and corals;† and, as Dr. Hinde himself admits, M. Renard distinctly states that there is no evidence that the infiltered silex into the limestone is derived from the decomposition of sponge-spicules or frustules of diatoms. Surely such a statement from so competent an observer is entitled to more consideration than that accorded to it by Dr Hinde, who considers that M. Renard has mistaken sponge-spicules for crinoid stems.*

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* *Loc cit.*, p. 492

† *Ibid.*, p 196

In conclusion, it may be asked what is the evidence which Dr. Hinde can assign for his statement—that the silica of Carboniferous chert has been derived from sponge-spicules? Absolutely none, except a fanciful analogy between these peculiar masses and the sponge-beds of the Cretaceous formation. On the other hand, it has been shown that no such analogy exists, inasmuch as there was a marked contrast between the organic beings in the waters of the Carboniferous seas as compared with those of the Cretaceous period. In the former siliceous sponges were exceedingly rare; in the latter they abounded; so that, whatever part they may have played in the construction of the Cretaceous bands of chert, it is clear they could have taken no important part in the formation of the chert-bands of the Carboniferous Limestone. The relative weight of opinion as expressed in the papers dealing specially with this subject must be left to individual judgment; in forming this judgment, however, it will not be overlooked that identical conclusions have been arrived at regarding the mode of formation of the Carboniferous chert-bands by two sets of observers working independently, one in Ireland the other in Belgium, almost at the same period, and both using chemical and microscopical appliances.

I trust, therefore, that I have succeeded in showing that there are good grounds for the opinion of those who consider that the beds and nodules of siliceous material in the Carboniferous Limestone have been formed by a direct replacement of original calcareous matter of the limestone itself by silica held in solution in the ocean-waters, and that, consequently, Dr. Hinde is not justified in referring them for their origin to sponge-structures

II. "Note on Professor Hull's Paper." By EDWARD T. HARDMAN, of the Geological Survey of Ireland. Communicated by E. HULL, F.R.S. Received April 5, 1887.

Dr. Alleyne Nicholson, a palæontologist of no small repute, refers to this subject in his work on the 'Ancient Life History of the Earth,' p. 34. He considers that the silica which has surrounded and infiltrated the fossils which flint contains, must have been deposited "from sea-water in a gelatinous condition, and subsequently have hardened." Also that "the formation of flint may, therefore be regarded as due to the separation of silica from sea-water, and its deposition round some organic body in a state of chemical change or decay."

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* Dr Hinde's words are · "There are shown, however, in one of the figures (fig. 2) accompanying M. Renard's paper, circular sections which more nearly resemble those of sponge-spicules than of crinoid stems, to which they are assigned." Note, *loc cit*, p 433

This is essentially the theory I advanced in our joint paper, and that independently arrived at by the Abbé Renard, namely, pseudomorphism.

Dr. Nicholson says further: "It has been asserted that the flints of the chalk are merely fossil sponges. No explanation of the origin of flint, however, can be satisfactory, unless it embraces the origin of chert in almost all limestones from the Silurian upwards, as well as the common phenomenon of the silicification of organic bodies (such as corals and shells) which are known *with certainty to have been originally calcareous.*"

In our paper the prevalence and thickness of the chert of the Carboniferous Limestone of Ireland is referred to. I have since had an opportunity of seeing the siliceous alteration of limestone on a very large scale, and in different formations, in the tropical region of Western Australia, when engaged there as Government Geologist. It is seen in the Lower Silurian, Carboniferous, and Upper Tertiary deposits. The transition from the limestones into chert, flint, and calcedony, is clearly visible in many places where these minerals form ranges often miles in extent, and where the thickness of the flinty material occasionally reaches 300 feet.

It is curious that these flint beds nearly always form the capping of the hills, but that they are of the same formation as the underlying limestone is proved by the gradual passage of that rock into flint; and where fossils occur in the limestone similar fossils are observed in the flint, until they become obliterated towards the summit.* I am inclined to attribute this to the action of highly-heated *rain-water* since the rocks have been deposited. In the warm season—which is also the rainy season, from about November to March—the rocks become intensely heated, and consequently, also the water lying in pools and cavities. I have been assured by settlers who have had to wade through flooded country, that at such times they could hardly endure the heat of the water, and I have experienced this to a slight extent myself. It is certain that under these circumstances silica would be more largely dissolved from one part, and more quickly deposited in another portion of the same rock; it is in fact on similar reasoning—the influence of sunlight and heat—that Professor Martin Duncan, F.R.S., explains the silicification of the West Indian Miocene Corals.

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* See 'Report on the Geology of the Kimberley District, W. Australia.' E. T. Hardman. Perth, W. A., 1885. P. 13.

Whether it be heated rain-water, or heated sea-water containing silica, the principle of the transmutation is the same.

These siliceous beds are found, not only in the marine Silurian (and possibly older) beds of tropical Australia, in which sponges are comparatively rare, and in the Carboniferous rocks, but also in a fresh-water deposit which caps a hill south of Mount Elder on the Ord River, and about 500 feet above the level of the country, showing that it must at one time have been the bed of a very extensive lake. The upper beds are white limestone merging upwards as usual into flint, calcedony, and green agates. These are 50 feet thick, and all abound in a fossil, *Planorbis*, as determined by Professor McCoy, of Melbourne University, who named it as a new species, *Planorbis Hardmani*. His decision was confirmed by R. Etheridge, Junr., and Dr. Woodward, and the specimens are at present in the Museum at South Kensington.

This rock is simply one mass of *Planorbis* shells all highly silicified. I can hardly conceive that it was formed from sponge spicules, especially as according to Ernst Haeckel ('History of Creation,' p. 139) the main class of the Sponges lives in the sea, with the single exception of the green fresh-water Sponge (*Spongilla*).

It is not probable then that these organisms would have existed in these regions in sufficient numbers to form a rock 50 feet thick and over two miles square at present.

We have therefore examples at both ends of the scale in this one country showing how improbable is the Sponge theory of chert.

III. "On the Homologies and Succession of the Teeth in the Dasyuridæ, with an Attempt to trace the History of the Evolution of Mammalian Teeth in general." By OLDFIELD THOMAS, British Museum (Natural History). Communicated by Dr. ALBERT GUNTHER, F.R.S. Received April 4, 1887.

(Abstract.)

The true homologies of the different teeth in the Marsupialia, and especially in the *Dasyuridæ*, have long been in a state of confusion, largely owing to their perplexing superficial resemblances to the teeth of the Carnivora and other Placentals, and to the incorrect homologies thereon founded. This confusion has been chiefly in regard to the premolars, of which some members of the family have two, others three, while generalised Placentals have four, and it is therefore necessary to prove which teeth have been successively lost in order to find out the correct homologies of the remainder.

Firstly, as to which of the three premolars of such genera as *Thylacinus* and *Phascologale* have been lost in *Dasyurus* and *Sarcophilus*, each with only two—a study of the different members of the genus *Phascologale* shows that, judging by the great variability in size of the last premolar or pm.^{4} of the typical mammalian dentition,* which is sometimes even altogether aborted, it is this tooth that is the one lost in *Dasyurus* and *Sarcophilus*, the total loss of the changing tooth naturally accounting for the non-discovery of a tooth-change in these genera.

Next, since the original number of premolars was clearly four in the Marsupials as well as in the Placentals, it was necessary to find out which of these had disappeared in the ordinary three-toothed genera of the Polyprotodonts, and this has been able to be done by the fortunate discovery of a specimen of *Phascologale* in which there are four premolars on one side, the additional tooth being inserted between the ordinary first and second premolars. The missing premolar is therefore pm.^{2}, as shown both by this instance and by the relative positions of the teeth in other Polyprotodonts, the resulting premolar formula of *Phascologale* and *Thylacinus* being P.M. $\frac{1.0.3.4}{1.0.3.4}$ and of *Dasyurus* and *Sarcophilus* P.M. $\frac{1.0.3.0}{1.0.3.0}$.†

The milk dentition in several of the *Dasyuridæ* is then described, among others that of the Purbeck Mesozoic Marsupial *Triacanthodon serrula* (Owen), which is proved to have, as had been suggested by Professors Owen and Flower, a milk dentition identical with that of the modern Marsupials.

An attempt is then made to trace out the history of the evolution of mammalian teeth in general, and as a preliminary it is insisted (1) that the rudimentary tooth-change of the Marsupials is not a remnant of a fuller one, but a low and early stage in the development of complete diphyodontism, a stage out of which the Eutheria have long ago passed; and (2) that, as maintained by Professor Flower, the milk teeth are the superadded and not the primary set.

It is then suggested that the process by which a milk tooth was developed consisted of two stages, firstly, a preliminary retardation of the permanent tooth, and secondly, of the development of a temporary tooth in the gap in the tooth-row caused thereby; the retardation in the first case being useful for packing purposes in a large-toothed animal, while in a small-toothed form the same retardation, if present by inheritance, would cause a more or less disadvantageous gap, best filled by the assumption of a milk tooth.

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* Although the homology of this tooth with the pm.^{4} of Placentals, first made out by Professor Flower, has been called in question, there can be no doubt that it is entirely correct.

† This method of writing dental formulæ is recommended as showing not only the total number, but the homologies of the teeth, each of which has its own number in the series.

The first stage, or stage of retardation, appears to be still represented in the anterior upper incisors of many Polyprotodont Marsupials, and it is therefore believed that these teeth now represent the stage at which the ancestors of the Marsupials and Eutheria diverged from one another, a stage at which the further development of milk incisors was just commencing.

Following out this idea, it is shown how easily the transition from the Metatherian to the Eutherian stage of tooth-change may have taken place, a transition by the help of which a complete series of diagrams can be drawn up, following the history of each individual tooth, from the dentition of the earliest Mammals, homodont and monophyodont, as no doubt the unmodified Prototheria were, down to the varied forms of dentition, heterodont and diphyodont, existing at the present day.

All the orders of Mammalia fall easily enough into their places in the main line of this scheme with one exception, namely, the Edentata, in whose case the evidence all tends to prove the correctness of Professor Parker's suggestion as to their nearly direct derivation from the Prototheria, a suggestion that the characters of their teeth most fully support. On the same principles, therefore, as the main Proto-meta-eutherian line of tooth development is drawn up, a side branch, for which the name "Paratherian" is suggested, is made for the Edentates. Within that branch very little heterodontism has ever been developed, but otherwise the changes, except in the case of the as yet inexplicable dentition of *Orycteropus* have been of the same nature as those in the main line, the superaddition of a milk set of teeth in *Tatusia* being, as in the Meta- and Eu-theria, the last and most highly specialised development.

IV. "Note on Protection in Anthrax." By L. C. WOOLDRIDGE, M.D., D.Sc., Demonstrator of Physiology, Guy's Hospital. Communicated by E. KLEIN, M.D., F.R.S. Received April 16, 1887.

Hitherto in the few cases in which protection against zymotic disease has been found possible, it has been effected by the communication to the animal of a modified form of the disease against which protection is sought.

I have succeeded in protecting rabbits from anthrax by an altogether different process, and although this is scarcely, at present, of practical utility, it may perhaps be found to be of some interest as regards the general nature of protection in this and other diseases depending on micro-organisms.

I use as a culture fluid for the anthrax bacillus a solution of a proteid body which is obtained from the testis and from the thymus gland. I have described this substance to the Society on a previous occasion,* so that I need not repeat the description of the process used in its preparation.

The proteid substance is dissolved in dilute alkali and the solution sterilised by repeated boiling. It is then inoculated with anthrax and maintained at 37° C. for two or three days.

The growth is generally not very abundant, and at the end of the period mentioned is removed from the culture fluid by filtration. A small quantity of the filtered culture fluid is injected into the circulation of a rabbit, and it is then found that the animal will not take anthrax.

A subcutaneous inoculation of extremely virulent anthrax blood made at the time of the injection of the protecting fluid, and two subsequent inoculations at intervals of five and ten days, remain entirely without effect. The animals used as a control invariably die. Four rabbits have been protected in this way.

If the anthrax grown in the fluid be inoculated it either kills or it has no effect. It does not protect in the slightest degree.

The injection of the culture fluid in which no anthrax has grown is without effect. The animals die as usual when inoculated. The injection of the fluid itself causes no ill symptoms whether anthrax has grown in it or not.

If other albuminous fluids, *e.g.*, blood-serum, be used as a culture medium and the filtered culture fluid be injected, it exerts no protection. It may be fairly concluded that the growth of the anthrax bacillus in the special culture fluids used in these experiments gives rise to a substance which when injected into the organism protects against an immediate and subsequent attacks of anthrax.

It would obviously be of very great advantage if some such method as this could be used for the zymotic diseases affecting man for which no protective inoculation in the ordinary sense, appears possible.

I am indebted to the Medical Officer to the Local Government Board for permission to publish this short account of these experiments, the full description of which will appear in his report. I must also express my thanks to Dr. Klein, F.R.S., for kindly supplying me with many anthrax cultivations.

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* L. C. Wooldridge, "Intravascular Clotting," 'Roy. Soc. Proc.,' 1886.

Note added April 27th.

The following experiments give additional weight to the previously described results.

In the one case the anthrax grew with very great rapidity in the culture fluid, and the clear filtrate contained but a very small quantity of proteid matter. Forty cubic centimetres of this fluid was injected into a rabbit, and the rabbit immediately inoculated in the ear with virulent anthrax blood; in two days there was very marked œdema at the seat of inoculation, which increased to an enormous extent during the next few days, and then gradually subsided. The rabbit is now perfectly well, twenty-four days after the inoculation.

In the second case the growth of anthrax had been very slight; 20 c.c. of the filtered fluid was injected, and the animal immediately inoculated in the leg with virulent anthrax blood. In three days there was marked œdema at the seat of inoculation. This spread up the leg to the back, so that there was enormous œdema occupying nearly the whole posterior part of the animal; this persisted for ten days, and then gradually subsided. The animal is quite well, twenty-eight days after inoculation.

These cases are of interest, since they are obviously instances of partial protection. The animals are still affected by anthrax, but it is only as a severe local affection, and does not kill them.

Presents, April 28th, 1887.

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London:—Royal Microscopical Society. Journal. December, 1886, and April, 1887. 8vo. *London* 1886–87. The Society.

Manchester —Geological Society Transactions Vol XIX Parts 6-7 8vo *Manchester* 1887 The Society

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Zurich —Schweizerisches Polytechnikum Verzeichniss der Bibliothek 8vo *Zurich* 1887 Prof R Wolf

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Observations and Reports

Chemnitz —Konigl Sachs Meteorologisches Institut Jahrbuch, 1885 4to *Chemnitz* 1886 The Institute

Liverpool —Free Public Library Thirty fourth Annual Report 8vo *Liverpool* 1887 The Committee

London —Royal Gardens, Kew Bulletin of Miscellaneous Information No 3 8vo *London* 1887 The Director

Tiflis —Physikalisches Observatorium Meteorologische Beobachtungen, 1885 8vo *Tiflis* 1886 The Observatory

Vienna —K K Universitats-Sternwarte Annalen Band IV Jahrgang 1884 4to *Wien* 1886 The Observatory

Washington —Smithsonian Institution Report 1884 Part 2 8vo *Washington* 1885 The Institution

U S Coast and Geodetic Survey Report 1885 Parts 1-2 8vo *Washington* 1886. The Survey

U S Geological Survey Bulletin Nos 30-33 8vo *Washington* 1886, Monographs Vol XI 4to *Washington* 1885 The Survey

May 5, 1887.

Professor G G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Buchanan, John Young, M A.
Cash, John Theodore, M D
Douglass, Sir James Nicholas, M.I.C E.
Ewing, Prof. James Alfred, B.Sc
Forbes, Prof. George, M.A.
Gowers, William Richard, M.D.
Kennedy, Prof. Alexander B. W., M.I C.E.
King, George, M B.
Kirk, Sir John, M D.
Lodge, Prof. Oliver Joseph, D Sc.
Milne, Prof. John, F.G S.
Pickard-Cambridge, Rev. Octavius, M.A.
Snelus, George James, F C S.
Walsingham, Thomas, Lord.
Whitaker, William, B.A.

The following Papers were read:—

I. "Report of the Observations of the Total Solar Eclipse of August 29, 1886, made at Carriacou." By the Rev. S. J. PERRY, S.J., F.R.S. Received April 5, 1887.

(Abstract.)

Carriacou is a small island situated about twenty miles to the north of the island of Grenada, the chief of the Windward group, and furnished an excellent site for the observation of the last solar eclipse. Most of the observers sent by the Eclipse Committee of the Royal Society to the West Indies in August of last year remained at Grenada, or on the small islands in its immediate vicinity, whilst Mr. Maunder and myself occupied the more distant northern station, where the totality was slightly diminished in duration.

The work proposed for Mr. Maunder was to secure a series of photographs of the corona, with exposures of 40s. and under, and also to obtain two photographs of the spectrum of the corona with the longest exposures possible. The coronal pictures were successful, and they are at present in the hands of Mr. Wesley, Assistant Secretary of

the Royal Astronomical Society. The results of his careful examination, and of the collation of this with other eclipse photographs, will form the subject of a later communication. The spectroscopic cameras mounted on the same equatorial framework failed to give any useful result.

The instrument used by myself was a 5½ inch equatorial by Alvan Clark with a Rowland grating 14,438 lines to the inch, and the work assigned by the Committee was an examination of the spectrum of the inner corona immediately before and after totality and a search for the carbon bands during totality. I was assisted by Sub-Lieutenant Helby, of H.M.S. "Sparrowhawk," who pointed the slit of the spectroscope whilst my undivided attention was given to the bright lines in the field of view of the small observing telescope. The object glass cast a most perfect picture of the corona on the white enamelled cap of the slit plate, so there could have been no difficulty in directing the slit to any assigned position.

In the search during totality for the carbon bands α and β, which should have been well within the field of view, extending as it did from wave length 5600 to considerably beyond *b*, the radial slit was placed near the solar equator, at distances from the moon's limb varying from 0·1 to 0·5 of a solar diameter, and was then removed to one of the sun's poles, and placed tangentially at successive distances as before. In none of these positions could I detect the slightest trace of the carbon bands. The conclusion indicated by these observations seems to be that the vapour of carbon, if present in 1886, was not of sufficient intensity to make an appreciable impression on the retina. The dispersion was considerable, as I was using the second order of spectrum with a power of about 4, but the fainter lines in the solar spectrum, and the coronal lines when seen, were so distinct, that I scarcely think the dispersion could have been excessive.

The observations both before and after totality were greatly interfered with by the clouds and heavy rain, but although rain fell within a few minutes from the beginning and also very soon after totality, the sun seemed perfectly clear during the whole of the totality itself. As soon as the sun broke through the clouds and became visible on the slit of the spectroscope, Lieutenant Helby placed the slit at the centre of the rapidly decreasing crescent, and the first line that I detected was 1474 K, which extended to a distance of perhaps 8′ from the limb. Almost at the same instant I saw a mass of lines of unequal length situated on the less refrangible side of *b*, but in close proximity to it. Their number I estimated at about 15, but I could form no idea of their relative intensities. This observation seems to favour the view that the absorption producing the Fraunhofer lines takes place in successive layers of the solar atmosphere, and not in any one layer exclusively. Within 20s from the end of totality, the radial slit being as near as possible to the point of reappearance, the whole field was crowded with bright lines, fifty or more being visible in the short space between wave-length 5600 and *b*. I noticed no difference in the length of these lines. Clouds and rain soon put an end to all chance of further observations.

Interesting sketches were made during totality of the outer streamers by Captain Masterman and Mr. Osborn, of H.M.S. "Bullfrog," who both used the circular disks arranged so as to cover the brighter portions of the inner corona. The instantaneous view that I obtained of the corona, most exquisitely defined on the white cap of the spectroscope, and the rapid glance I took with an excellent binocular, confirm the positions of the two principal rays drawn by Captain Masterman, but I observed at the same time a shorter ray between the two, which appears otherwise to have escaped detection, and I noticed the leaf-shaped curvature of the ray in the north-west.

The darkness was never much less than that of a fair moonlight night.

II "Note on the Microscopic Structure of Rock Specimens from three Peaks in the Caucasus." By T. G. BONNEY, D.Sc., LL.D., F.R.S., Professor of Geology in University College, London. Received April 5, 1887.

Although our knowledge of the petrology of the Caucasus has been considerably augmented of late years through the labours of Abich, Favre, Tschermak, and others, so much ground still remains untrodden among its mountain peaks that hardly any specimen can be entirely without interest. Those described in the present note have come from the following localities:—(1) The summit of Tau Tetnuld; (2) rocks from the upper part of Guluku; (3) the summit of Elbruz.

The specimens from Tau Tetnuld and Guluku were collected by Mr. W F. Donkin, during his expedition in company with Mr. Clinton Dent in the summer of 1886, and to the former I am indebted for the following note on the localities.

"Tau Tetnuld is one of the peaks of the central Caucasian chain, in the great Koschtan-Tau group which lies about midway between Elbruz to the N.W., and Kasbek to the E S.E. From Koschtan-Tau the main ridge forming the watershed runs somewhat north of west, dropping gradually in height; but for some three or four miles forming a magnificent wall on the northern side, covered with a succession of steep snow-slopes and hanging glaciers A long portion of this ridge, including three more or less well-marked elevations, is called Djanga; the next elevation on the ridge—a much more obvious one, forming indeed a symmetrical snow pyramid—is Tau Tetnuld.

From the north it appears to have a sharp conical summit, but it is really wedge shaped. Further westwards the ridge falls continuously, a few rocky peaks protruding from it, to a well marked snow col. The drainage from the whole of this vast wall from Koschtan-Tau to the col inclusive, collects in a basin, and flows northwards as the Bezingi glacier. The glacier is remarkably level and free from ice falls, and appears to start almost direct from the foot of the wall, with but little sloping névé. Its course is soon narrowed to a channel of some 1200 yards wide by spurs from the high ridges running northwards on either side. On the west the ridge does not attain any great elevation, but on the east the glacier is bounded by a group of mountains culminating in the great rock mass of Guluku. This group is completely separated from the Koschtan Tau chain by the glacier basin above mentioned. Guluku itself is granitic in character, but the lower and surrounding peaks and ridges are schistose and shaly, in parts exactly like the Ober and Unter Rothhorn on the north side of the Findelen glacier.* The upper rocks of Guluku are grey in colour and look very like granite from some distance below, then comes a belt of whiter rocks (A), and below that a well marked red belt (B), both these belts are continuous and nearly horizontal for a long way round the southern side of the peak. Below the red belt the rocks are darker and more mixed (C, D, and E). On the moraine on the east side of the Urban glacier, under Guluku, vast masses of granite had fallen, many of the blocks recently. It is fine grained, and of grey colour (F).'

(1.) *Tau Tetnuld.*—Specimen from the highest rocks about 100 feet below the actual summit, which was covered with snow. Mr. Donkin states that the rock traversed in the ascent appeared to be exactly of the same character. This is a flat fragment of a brownish, rather fissile, but strong, mica schist, about ⅛ inch thick and nearly 2 inches broad and long. The broad surfaces are spangled with small flakes of a silvery mica, and appear to be those of a 'cleavage foliation." Examined microscopically, the chief constituents are quartz and mica, besides which an iron oxide occurs, frequently in small granules and rods, and more rarely in larger grains. The quartz and mica have a general elongated lenticular arrangement parallel with the broader surfaces of the fragment, and cracks traverse the slide in the same direction. The quartz on applying the polarising apparatus is broken up into a mosaic of different sized grains united by diverse tinted margins, so that we are evidently dealing in each case with one or more grains which have been crushed up and re-cemented. Cavities are sometimes rather numerous, and occasionally tend to range themselves perpendicularly to the lines of cleavage They are generally minute, sometimes stained internally, both ovoid and irregular in form, usually containing fluid, and with bubbles which, as a rule, are about one fourth the volume of the cavity, but not rarely exceed this Some of the large grains exhibit the usual indications of being in a state of strain.

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* Near Zermatt in the Pennine Alps. These schists are referred to the uppermost group (Graue kalkhaltige Schiefer) in the crystalline series of the Alps.

The mica is brown, greenish, or colourless The first and second are biotite, more or less altered The colourless mica resembles muscovite, but I think that at any rate some of it is a magnesia-potash mica, possibly hydrous, a secondary product after biotite, the iron having separated out This often remains between the cleavage planes in rods and plates. Possibly some of the smaller flakes of mica may be altogether of secondary origin, but I have no doubt that most of it, including all the larger flakes, is an orginal constituent. These flakes often afford marked evidence of mechanical disturbance. They are bent, twisted, crumpled, and in some cases crushed up. Portions of them, viewed with the polarising apparatus, have a peculiar "powdered" look, which I find very characteristic of a mica that has been to a certain extent crushed *in situ*, so that, while the general outline of a crystal is preserved, there are constant ruptures of continuity and slight displacements of the constituent parts. A few small mineral granules also occur in the slide, some I am disposed to refer to epidote, others to a very impure garnet.

(2.) The specimens from Guluku were collected, partly *in situ* at a height roughly of 14,500 or 15,000 feet above the sea, partly from a moraine, as above-mentioned, on the Urban glacier; hence they represent a considerable mass of the mountain below the level just mentioned. The highest point of Guluku is about 16,500 feet above the sea.

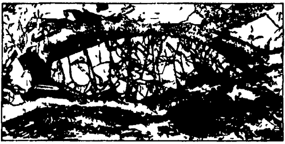
(A.) From the highest rocks reached. A small fragment of rock with indications of a slight cleavage, consisting of a porcelain-white mineral irregularly mottled with one of a pale pistachio-green colour. The former on microscopic examination proves to be a plagioclastic felspar, considerably decomposed, but in parts showing very clearly a lamellar twinning. The extinction angles are generally rather small, probably oligoclase predominates; microlithic flakes of a micaceous mineral, and other decomposition products, are frequent. The other mineral is an epidote, varying from a pale yellow tinge to colourless, and rather impure It occurs in aggregated and sometimes rather fan-like groups of longish crystals. There are also a few spots of a serpentinous or chloritic mineral. The mechanical disturbance of the rock is obviously posterior to the crystallisation of the felspar, as its crystals are cracked and even sheared, but, at any rate in the main, prior to that of the epidote.

Guluku (B).—A small fragment of a coarse gneissose rock, evidently containing a considerable amount of a darkish mica, on the weathered surfaces reddish brown (the "red band") Under the microscope it is seen to consist chiefly of biotite, felspar, considerably decomposed, in part at least plagioclase, and quartz The biotite is in places altered into a greenish chloritic mineral, in others is "bleached" by parting with its iron A white mica however, which occurs in good sized flakes, appears to be an occasional original constituent The rock has evidently been much crushed The quartz is cracked and displaced, the felspar has been broken up and parts of the original crystals are now occupied by a sort of irregular mosaic or mixture of felspar, quartz, kaolin, and white mica The felspar crystals are occasionally interrupted by roundish inclusions of quartz, such as one often sees in the oldest gneisses These may be of secondary origin but I find nothing to prove it The original quartz grains, where adjacent to the crushed felspar, appear to have been augmented by secondary deposits of quartz in optical continuity The mica in parts of the slide shows marked indications of mechanical disturbance and a reddish garnet at the edge has been distinctly crushed out as is more fully described in (C)

Gululu (C) —This specimen, in shape roughly a right rhomboidal prism about 1 × 1 × ½, has for its larger faces parallel joint surfaces two others are sheen surfaces,' parallel with which the fractured faces exhibit a foliated structure The rock appears to be a strong rather compact mica schist, dark in colour, with a few very thin lighter-tinted bands

The principal minerals are quartz, mica, garnet, iron oxide and a quantity of a brownish mineral, sometimes very fibrous The quartz occurs mostly in granules of moderate size, occasionally including a little minute rutile (?) and mica It is on the whole fairly clear, but here and there cavities are pretty numerous These frequently contain bubbles which, though very variable in relative size are generally smaller than in the Tau Tetnuld rock, perhaps commonly about one sixth or one seventh of the whole volume The mica constituent is chiefly biotite or its alteration products, often a greenish chloritic mineral, sometimes a whitish hydrous mica, both with interlamination of iron oxide The garnet is colourless in thin slices, and occasionally exhibits, along cracks, alteration for a short distance into a chloritic mineral Some of the larger granules of iron oxide are hematite The pale coloured rather filmy or fibrous mineral is certainly in some cases a secondary product after a felspar, the usual aggregate of a minute micaceous or kaolinitic mineral Other parts, however, consisting of narrow undulating bands of an aggregated fibrous mineral, like a small lock of wavy hair, I was at first disposed to regard as fibrolite, but after repeated examination I am unable to decide They resemble in some respects a fibrous mica, but their extinction does not appear to agree with this mineral (though accurate measurements are difficult to obtain) for it seems markedly oblique.

It is evident at a glance that this rock has been subjected to a great pressure normal to the conspicuous foliation. The garnets have been cracked and crushed out, so as to have become elongated ovals in shape. A glance at the diagram will render a more minute description needless, and will show that the garnet was more or less flattened out before it broke. The quartz grains also are cracked, being sometimes only a little, sometimes much displaced. This mineral, however, does not appear to have been so completely crushed up as in the Tau Tetnuld schist. Occasionally a small grain, adjacent to the (original) felspathic constituent, has escaped altogether. The felspar has, I believe, often been crushed out, and then converted into the above-named microlithic mineral. The larger mica flakes are twisted about in the manner usual in a rock which has been crushed. Study of this slide seems to me to show conclusively that this rock, anterior to the crushing, was a moderately coarse crystalline rock, consisting chiefly of quartz, felspar, biotite, and garnet, probably a rather micaceous gneiss.



Garnet, squeezed out and cracked; surrounded by biotite, quartz and the fibrolitic mineral. × 25 diam.

Guluku (D).—A small fragment of a micaceous granitoid rock.

Except for the greater abundance of biotite and the smaller amount of quartz this rock is closely allied to the next described; the felspar is a little more decomposed, and small garnets are rather more numerous. With these modifications the description given below applies here, and this rock, too, has evidently undergone about a similar amount of mechanical disturbance. Another small fragment from about the same level contains more white mica, but as the general aspect suggests no important difference, and it is not a very promising specimen, I have not had a section made. This occurs at a slightly lower level than (B), and the two are about 100 feet below (A).

Guluku (F).—Section from one of two specimens representing numerous large blocks of granite fallen from west side of Guluku.

This is a fairly coarse grained rock chiefly consisting of a white felspar and dark mica, not rich in quartz. The mica is mainly biotite in good preservation. There is also a certain amount of a white mica which appears to be an original constituent and to belong to the muscovite group. The felspar is occasionally replaced by kaolinite and micaceous minerals, but much of it is in good preservation, sometimes one part of a crystal is reduced to an 'earthy' condition, while the rest is quite fresh. Most of the crystals show the twinning of plagioclase, generally on the albite type, but occasionally on the pericline. One of the grains appears to be microcline. I have measured the extinction angles of several parallel lamellæ; it is difficult to get very satisfactory results, but, as in two of the best cases, they appear distinctly too large for albite, between 30° and 40°, the felspar is probably oligoclase. There are two or three small colourless garnets, with probably a little apatite. The quartz contains cavities, in which small bubbles are usually present, about one sixth or one-seventh of the volume.

This rock has evidently been subjected to a certain amount of mechanical disturbance since its consolidation. The quartz grains are cracked and show strain polarisation. The felspar lamellæ are occasionally bent, now and then cracked across, but the effects are slight compared with the other cases.

Mr. Donkin also collected a specimen some miles further down the valley which resembled the rock in a neighbouring cliff. This appears to be a reddish rather fine grained felspathic granite, but as it was not obtained *in situ* I have not had a slice prepared.

The evidence of these specimens does not appear sufficient to warrant any positive statement as to the origin of these Caucasian rocks. The structure of one (D) seems rather to favour the idea of its having been a true granite (*i.e.*, an igneous rock), the same is true also of (F), while in another (B) there is a structure, which I have some reason to think characteristic of the Archæan gneisses.* But in the present state of our knowledge it would be unsafe to rely too much upon the latter criterion, because we do not yet know what modifications may be introduced by subsequent rearrangement of mineral constituents. It has indeed been proved in the case of hornblendic rocks, that the original structures, characteristic of crystallisation from a state of fusion, may be wholly obliterated, the same may take place also with granitic rocks. Still, even if this mode of metamorphism has occurred, there is some reason to believe that it dates usually, if not invariably, from a very remote period. We can, however, in my opinion venture to assert that these Caucasian rocks, after they had assumed a crystalline condition, underwent great pressures regional rather than local in their operation, which to some extent crushed the constituents and gave rise to certain mineral changes. It seems then a legitimate inference that in this part of the Caucasus, as in the Alps, the fundamental rocks consisted of crystalline rocks of more than one type, at a period long anterior to the operation of the pressures which folded this part of the earth's crust and upreared the mountain range.

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* The same difficulty exists in the case of some of the more highly crystalline rocks of the Alps. Favre ('Recherches—Chaîne du Caucase,' p. 70) states that the central part of the Caucasus is 'granite' which he compares with the protogine of the Alps, with a considerable belt of crystalline schists on the north and an intermittent one of the same, followed by slates, on the south.

• (3) The huge mass of Elbruz appears to consist mainly of volcanic rock, and is crowned by two crater peaks almost equal in height. Of these the eastern which is believed to be very slightly the lower of the two was ascended for the first time on July 31st, 1868 by Messrs Freshfield and Moore. On this occasion the western summit was so entirely concealed by clouds that its existence was not even suspected. The western summit was first ascended on July 28th, 1874, by Messrs Grove and Walker. Its "crater considerably exceeds in size that on the twin summit, and is probably about ¾ of a mile in diameter. The wall is perfect for some two thirds of its former circuit, but on the south west side a vast piece has fallen away, and a great glacier now flows down from the gap." The little peak forming its highest point, juts up on the north eastern segment of the limb. Its height above the sea, according to the Russian survey, is 18,526 feet, the eastern summit being 95 feet lower. The col between the two summits, according to Mr Grove, is about 17,350 feet. The specimen collected by Mr Walker was from the highest rocks traversed on the western peak, perhaps about half way between the col and the summit. It is a rough slab of a grey lava, with occasional small irregularly shaped vesicles, and scattered crystals of a whitish felspar up to about ¼ inch in length. The weathered parts are of a lightish-brown colour.

Microscopic examination shows that the rock has a clear glassy base crowded with minute lath like felspar microliths, apparently oligoclase, and occasional specks of opacite and aggregates of ferrite; possibly some minute granules of a pyroxenic mineral are present. To the same epoch of consolidation may belong some occasional elongated crystals of a light-coloured hornblende, but this is uncertain—there are a few grains of iron oxide, probably hematite. The larger crystals in the slide certainly belong to an anterior consolidation—these are (1) a dark brown hornblende, often with rounded outline, and sometimes blackened with included opacite, (2) a felspar, which generally resembles labradorite or andesine, but in one or two cases may possibly be sanidine. It is often rounded or broken in outline, is always greatly cracked, and contains many inclusions of a pale brown glass. One grain, indeed, consists very largely of glass, in which the crystalline parts are, so to say, embedded. This suggests that the mineral has been melted down *in situ* along the lines of natural fracture, rather than that it has incorporated the glass in crystallising. There are occasional cavities in the felspar, with bubbles varying in their relative size, which do not move. Grains of quartz, as observed by Tschermak ('Mineral. Mittheil.,' 1872, p. 108) in specimens brought by Favre from the lava streams lower down the mountain, do not occur in this specimen. A fluidal structure is barely indicated. The rock may be named a hornblende-andesite. I have compared the slide with one from the upper part of Ararat (lent me by Professor Judd), and with my own collection of andesites and allied rocks from Auvergne, Germany, Hungary, Italy, Old Providence Island, and the Andes, but it differs varietally from all.

III. "On the Distribution of Strain in the Earth's Crust resulting from Secular Cooling, with special Reference to the Growth of Continents and the Formation of Mountain-chains."

By CHARLES DAVISON, M.A., Mathematical Master at King Edward's High School, Birmingham. Communicated by Prof. T. G. BONNEY, D.Sc., F.R.S. Received April 7, 1887.

(Abstract.)

The paper is founded on—

1. Sir W. Thomson's and Professor G. H. Darwin's researches on the rigidity of the earth.
2. Sir W. Thomson's investigation on the secular cooling of the earth.
3. The contraction theory of mountain formation.

I. *The Distribution of Strain in the Earth's Crust resulting from Secular Cooling.*

The following problem is solved:—A globe, of radius r, is surrounded by a number of concentric spherical shells, called A_1, A_2, A_3 . . . ., of thickness a_1, a_2, a_3 . . . . respectively. The globe remaining at its initial temperature, the shell A_1 is cooled by $t_1°$, the shell A_2 by $t_2°$, in the same time, and so on. The linear coefficient of expansion being e, and the same for all the shells, it is required to find the distribution of strain resulting from this method of cooling.

An expression is found giving the change of radius of the inner surface of any shell. Supposing all the shells to be of equal thickness a, the change of radius of the inner surface of the shell A_{n+1} is proportional to

$$\frac{e}{(r+na)^2}\left[(r+na)^3(t_{n+1}-t_n)+(r+\overline{n-1}\,a)^3(t_n-t_{n-1})+\right.$$
$$\left.+(r+a)^3(t_2-t_1)+r^3t_1\right] \qquad (1)$$

i.e., if the shells be infinitely thin, to

$$\frac{e}{(r+na)^2}\int_r^{r+na} x^3\ \frac{dt}{dx}\,dx, \qquad (2)$$

t being proportional to the rate of cooling of any shell.

If this expression be positive for any shell, the shell is stretched; if negative, it is crushed or folded.

To apply this problem to the case of the earth, the law of cooling taken is that which follows from Sir W. Thomson's solution, in his memoir on the secular cooling of the earth. The expression in the form (2) proves unserviceable, and therefore the expression (1) is made use of as follows:—

Taking the time since solidification provisionally at 174,240,000 years, it is shown that the rate of cooling (dv/dt) is practically insensible at a depth of 400 miles. The radius of a sphere equal to the earth in volume being about 3959 miles, the earth is supposed to be constituted as follows:—A central globe 3559 miles in radius, at the initial temperature of the earth, which as yet has not sensibly cooled, surrounded by 400 concentric spherical shells, each one mile in thickness, the rate of cooling in each shell being uniform throughout, and equal to its value at the outer surface of that shell.

The results of the calculation are shown by the curve in the figure accompanying the paper, and the following conclusions are deduced, taking the time since consolidation provisionally at 174,240,000 years:—

1. Folding by lateral pressure takes place only to a certain depth (about five miles) below the earth's surface, and below this depth changes to stretching by lateral tension.

2. Stretching by lateral tension, inappreciable below a depth of 400 miles, increases from that depth towards the surface; it is greatest at a depth of 72 miles (*i.e.*, just below the depth at which the rate of cooling is greatest), after this it decreases, and vanishes at a depth of about five miles.

3. Folding by lateral pressure commences at a depth of about five miles, and gradually increases, being greatest near the surface of the earth.

No great importance is attributed to the numerical results. The conclusions are given for their qualitative rather than their quantitative value. They depend also on the assumption that the earth's surface is smooth and spherical.

The following laws are also shown to be approximately true:—

1. The depth of the surface at which folding by lateral pressure vanishes, and the depth of the surface at which stretching by lateral tension is greatest, both increase as the square root of the time that has elapsed since the consolidation of the globe.

2. Folding by lateral pressure was effected most rapidly in the early epochs of the world's history as a solid globe; and since then, the total amount of rock folded in any given time decreases nearly in proportion as the square root of the time increases.

3. A law, similar to No. 2, for stretching by lateral tension.

II. *The Rev. O. Fisher's Argument on the Insufficiency of the Contraction Theory.*

The argument is described (see 'Phil. Mag.' for Feb., 1887). It is shown to be inconclusive on the following grounds:—

1. It assumes that the cooling of the earth to its present condition was instantaneous.

2. If instantaneous cooling were possible there would, it is shown, be no folding at all, but only stretching by lateral tension.

3. It assumes that the earth's surface was initially smooth and spherical, whereas Professor B. Peirce and Professor G. H. Darwin have both shown that vast continental wrinkles would be formed on the surface of a once viscous earth by the diminishing velocity of rotation resulting from tidal friction.

III. *The Effects of Crust-stretching and Folding on the Evolution of the Earth's Surface features.*

1. Owing to the pressure of the continental masses, crust stretching by lateral tension takes place principally beneath the ocean-basins, therefore deepening them and contributing to their permanence. This effect must have been greatest in early geological periods, when the surface of greatest stretching was close to the surface of the earth.

2. In another part of the paper it is shown that the amount of crust-stretching is considerably greater than the amount of crust-folding, due directly to secular cooling. Folding beneath the ocean-bed will therefore do little but diminish its rate of subsidence. The effects of folding in changing the forms of the earth's surface features must be most apparent in continental areas, especially along those coasts where the slope towards the ocean-depths is most rapid (*i.e.*, in the districts where earthquake and volcanic action are known to be most prevalent). In the coast regions, also, the products of conti-

nental denudation are chiefly deposited Hence, the continents grow by the formation of mountain-chains along their borders

3 The rate of mountain-making, and therefore also that of continental evolution, diminishes with the increase of the time

IV "Note on the Geological Bearing of Mr Davison's Paper" By T G BONNEY, D Sc, LL D, F R S, Professor of Geology in University College, London Received April 7, 1887

The results obtained by Mr Davison throw light upon one or two matters in regard to the petrology of the older rocks, which have always appeared to me difficult of explanation I venture therefore to add a brief note to his paper, written from the point of view of a geologist He throws light especially on the following matters —

(1) Among the older rocks the great foldings and their results, such as cleavage, appear to have occurred when the beds formed the upper layers of the earth's crust Thus the Ordovician rocks of North Wales were cleaved anterior to the deposit of the Silurian, the Carboniferous, and other Palæozoic rocks of South-west Britain and Britanny were plicated and cleaved, geologically speaking, shortly after their deposition The great foldings in the Scotch Highlands occurred, in great part at least, in Silurian time The disturbance of the Lake District rocks resulting in cleavage, must be placed between the end of the Silurian and the very beginning of the Carboniferous, that of Southern Scotland, between perhaps yet narrower limits The first epoch of mountain making in the Central Alps, with its plication and cleavage, immediately followed the deposition of the Eocene rocks The list might easily be extended

(2) The crystalline substratum often appears to be less modified than the overlying softer and more recent beds This I had attributed to the greater resistency of the former, but then could not see how to explain the foldings of the latter, if the others were comparatively uncompressed This, however, accords with Mr Davison's results of the diminishing effects of compression, while the fact that in early geological times the "neutral zone" between compression and tension was comparatively near the surface of the earth, may explain the frequent parallel arrangement of the minerals in the older Archæan gneisses I do not now refer to the more marked changes, such as the intercalation of calcareous or micaceous rocks, of more felspathic or quartzose layers whereby a stratification is simulated, if it be not recorded, but to the fact that very often a general parallelism may be

noted in the flakes of mica or any other mineral of somewhat like form scattered through the mass of the rock, sometimes approximating to a banding of the constituents, without any indication of this being the result of crushing. In regard to this particular structure, it is worth notice that it often lies in planes making a low angle with the horizon.

(3.) The same result may help to explain the assertion so frequently made, that among the older rocks the foliation (or minor mineral banding) is commonly parallel to the (apparent) stratification (or major mineral banding). This also I have noticed in cases where either there was no indication of subsequent crushing, or the latter had not effaced, and its effects could be distinguished from the earlier structure of the rock. I once supposed this parallelism and tendency to horizontality to be due to the weight of superimposed beds, but for some time have been dissatisfied with this explanation, because I could find no evidence that any heavy burden had been laid upon the older rocks till long after they had assumed a foliated structure. Tension, however, would probably produce the structure at least as readily as pressure, and the former of course would, as a rule, act parallel with the surface of the earth's crust, while compression should be exhibited commonly in planes making a high angle with it.

V. "Note on some Experiments on the Viscosity of Ice." By J. F. MAIN, M.A., D.Sc. Communicated by Prof. W. C. UNWIN, F.R.S. Received April 13, 1887.

(Abstract.)

The paper contains an account of some experiments on the continuous extension of bars of ice subjected to tension, made during the last winter in the Engadine. To eliminate the influence of regelation, the experiments have been carried on at such low temperatures as preclude the possibility of any effect being produced by this cause. The highest temperatures during the experiments were −2·6° C. in Experiment I., −1·0° C. in Experiment II., and −0·5° C. in Experiment III. These maximum temperatures only obtained for a very short time on one or two days.

The bars were tested in a compound lever testing machine with accurate knife edges, the load being a known weight of shot. The whole apparatus was enclosed in a double wood box. A delicate thermometer graduated to tenths of degrees, attached inside the box, gave the temperature at any given time, and the range of variation of temperature was recorded by two maximum and minimum thermometers, fixed inside to the roof of the inner box. To obtain ice

free from air water was boiled and then frozen It was then melted and again frozen in a mould Some difficulty was found in holding the ice bars in the testing machine The mode which answered best was to freeze the ends of the ice bar into conical metal collars, which fitted the shackles of the machine Extensions were measured by vernier callipers reading to one fiftieth of a millimetre between marked points on the metal collars To determine if any appreciable effect was due to distortion of the enlarged ends of the bars in the metal collars pieces of paper were gummed on the ice and the extensions also measured between fine pencil marks on these pieces of paper It was found that nearly all the stretching observed in measuring between the metal collars was due to stretching of the bar of ice, and only a very small part to shearing action in the collars In consequence of rapid evaporation from the surface of the ice bar, the stress with a fixed load on the lever increased from day to day

Three experiments are given on bars initially about 234 mm in length loaded to stresses of from 4 3 to 2 0 kilos per square cm and lasting from four to nine days

The three experiments show that ice subjected to tension stretches continuously by amounts which depend on the temperature and the tensile stress When the stress is great and the temperature not very low, there are extensions amounting to 1 per cent of the length per day So continuous and definite is the extension, that it can even be measured from hour to hour These extensions took place at temperatures which preclude the possibility of melting and regulation

The author hopes that on resuming the experiments next winter at St Moritz he may be able to determine more exactly the law of the extension He has shown already that the extension increases continuously with all stresses above 1 kilo per square cm, and at all temperatures between −6° C and freezing When ice is in a condition such that the point of a needle will cause a set of radiating fractures to pass from the point of contact in all directions, it stretches as certainly, though not by so great an amount, as when it will permit the passage through it of the same needle without showing flaw or scar

In the first experiment there was a total extension of 11 mm in nine days, in the second of 1 8 mm in five days, in the third of 1 7 mm in three days If we assume the extension proportional to the time, there was a mean daily extension of 1 2 mm, 0 36 mm, and 0 56 mm respectively The stress in No 1 was greater than in Nos 2 and 3 and the temperature not so very low in the day, though low at night In No 3 there was a low stress, but comparatively high temperature

VI. "The Tubercular Swellings on the Roots of the Leguminoseæ." By H. MARSHALL WARD, M.A., F.L.S., Fellow of Christ's College, Cambridge, and Professor of Botany in the Forestry School, Royal Indian College, Cooper's Hill. Communicated by Prof. M. FOSTER, Sec. R.S. Received April 25, 1887.

(Preliminary Note.)

The author finds that the tubercles on the roots of the Leguminoseæ are due to the action of a parasitic fungus. Not only has he produced the tubercles by infection from without, but he has also found the infecting agent, and repeatedly seen and figured the infecting hypha passing down inside a root-hair and across the cortex of the root into the young tubercle. Here the hyphal branches bud off yeast-like cells, which are extremely minute and numerous, and resemble bacteria at first sight; they differ in their mode of multiplying by budding.

The action of these minute germ-like bodies causes the protoplasm of the cells of the root to assume plasmodium-like characters, and induces the flow of nutritive substances to these cells, and hypertrophy results. On the decay of the tubercles, the germ-like bodies pass into the soil (where they can always be found) and infect other roots; it is very probable they may be of extreme importance in agriculture.

VII. "The Proteids of the Seeds of *Abrus precatorius* (Jequirity)." By SIDNEY MARTIN, M.D. Lond., Fellow of University College, London, and Pathologist to the Victoria Park Hospital. Communicated by Prof. E. A. SCHAFER, F.R.S. (From the Physiological Laboratory, University College, London.) Received April 21, 1887.

The proteids of the seeds of Abrus, the Indian liquorice, are important physiologically, because they have been shown (by Warden and Waddell*) to be possessed of poisonous properties. To the poisonous product extracted by these observers the name "abrin" was given; and though it was decided that abrin was closely allied to "plant-albumin," yet no experiments were recorded to show whether the product was a mixture or a single proteid. They obtained it by making a watery extract of the crushed seeds and precipitating with alcohol, the precipitate being afterwards collected and dried.

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* 'The Non-bacillar Nature of Abrus poison.' By C. J. H. Warden and L. A. Waddell. Calcutta, 1884.

Before proceeding to an examination of the physiological action of the jequirity, it seemed to me desirable to determine the kind of proteids present in the seeds, and the present communication embodies the results of the inquiries made with a view to such determination.

Method of Extraction of the Proteids.

The method used was based on the supposition that the proteids present in Abrus were similar to those in other seeds, consisting chiefly of proteids of the globulin and albumose classes.

The finely ground seeds were shaken first of all with chloroform to remove the red cuticle which sinks in this liquid, so that the yellow kernel powder could be readily removed and obtained in the dry state by allowing the chloroform to evaporate.

The powder obtained was then extracted with 15 per cent. sodium chloride solution for twenty-four hours and the mixture filtered. The yellowish filtrate was distinctly acid and gave a copious precipitate on boiling. The proteids were separated from this filtrate in two ways:—

(1.) Saturation with neutral ammonium sulphate and shaking for four hours throws down all the proteids in solution, the filtrate, after saturation, giving none of the proteid tests.

(2.) Saturation with sodium chloride and shaking for many hours gives only a scanty precipitate, which becomes copious on adding a large excess of glacial acetic acid. All the proteids are only with difficulty precipitated by this mode of saturation even after prolonged shaking.

Since ammonium sulphate so readily throws down all the proteids in solution, the precipitate caused by it was used in the following manner in the examination of the proteids:—The precipitate was collected and dissolved by adding distilled water, and the solution dialysed in running water (with thymol) for five to seven days.

Dialysis caused a copious precipitate which was collected and washed with distilled water (previously boiled to remove carbon dioxide) until no proteid in solution was present in the washings. The precipitate was then dried over sulphuric acid. The residue was in dark brown scales. It consisted of globulin with some colouring matter.

It is not possible to remove all the globulin by dialysis, so the liquid, after dialysing for seven days, was filtered into rectified spirit, which precipitated the remaining proteids. After standing under the alcohol six to eight weeks the globulin was coagulated, and the precipitate was collected, dried, and treated with distilled water, which dissolved out a proteid This proteid is an albumose The chloride of sodium method may be used instead of the ammonium sulphate, it takes a longer time, but gives products freer from colouring matter

For chemical examination, the albumose is readily prepared by boiling and filtering an aqueous infusion of the seed The globulin is coagulated while the albumose remains in solution

Properties of the Globulin

1 It is insoluble in distilled water, but readily soluble in 10 to 15 per cent sodium chloride or magnesium sulphate solution, soluble to a less extent in 5 per cent sodium chloride solution, and scarcely at all in 0 75 per cent

2 It is completely precipitated from solution by saturation with sodium chloride after slightly acidifying, and with ammonium sulphate, whether the solution be neutral, acid, or alkaline

3 It is coagulated by heat in 10 per cent magnesium sulphate solution, between 75° and 80 C, the liquid being made distinctly acid, in 10 per cent sodium chloride, between 66° and 73° C

4 When the solution in 10 per cent sodium chloride is placed in the incubator at 35° to 40° C, and allowed to remain twenty four or even forty eight hours, no precipitation occurs, a reaction in marked contrast to that given by some vegetable globulins In its high coagulation temperature, and in its non-precipitation from solution by prolonged exposure to a moderate heat, abrus globulin agrees with the proteid I have described in the juice of the fruit of *Carica papaya*, which, from its resemblance to serum-globulin, I have called vegetable paraglobulin * The vegetable myosins occurring in the cereals, wheat, rye, and barley, have a lower coagulation temperature than the paraglobulins, viz, 50°—55° C, and are precipitated from solution and rendered insoluble by a prolonged exposure to a temperature of 35°—40° C †

Properties of the Albumose

1 Soluble in cold or boiling distilled water Its chemical and physical properties are not apparently altered by boiling its solution

2 It is not precipitated from solution by saturation with sodium chloride unless a large excess of glacial acetic or phosphoric acid be added It is readily precipitated by saturation with neutral ammonium sulphate

3 It does not form an albuminate

4 Nitric acid does not precipitate it in a watery solution, but a precipitate falls if solid sodium chloride be added nearly to saturation

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* 'Nature of Papain &c Journ of Physiol,' vol 6 p 353

† Physiol Soc Proc ' Feb 12 1887

5 Acetic acid causes a cloudiness, which is increased by potassium ferrocyanide

6 Copper sulphate and basic acetate of lead cause precipitates, soluble in excess, mercuric chloride, a precipitate insoluble in excess

7 Copper sulphate and potash give a pink coloration (biuret reaction)

For the albumoses occurring in the vegetable kingdom I have proposed the name *phytalbumoses*, as they differ in many respects from the animal varieties

The phytalbumose in Abrus is closely allied to Kuhne and Chittenden s deutero albumose,* and identical with the α phytalbumose occurring in the papaw juice †

There are therefore, two proteids in the seeds of *Abrus precatorius*, a vegetable paraglobulin and α phytalbumose In conjunction with Dr Wolfenden I am now engaged in investigating the physiological action of each of these proteids, and hope soon to publish the results For the present it will be sufficient to call to notice the close resemblance between the proteids of the papaw juice and those of jequirity, since their physiological action appears to be in many respects similar

VIII "On the Diameters of Plane Cubics Preliminary Notice" By J J WALKER, F R S Received April 21, 1887

I showed some time back ('London Math Soc Proc,' vol 10, pp 184–5) that the Newtonian diameters of a plane cubic (u) envelope a conic, called hereinafter its "centroid," the equation of which, if in the system of co ordinates chosen the line at infinity be

$$\xi x + \eta y + \zeta z = 0,$$

is generally

$$\xi^2\left\{\frac{d^2u}{dy^2}\,\frac{d^2u}{dz^2} - \left(\frac{d^2u}{dy\,dz}\right)^2\right\} + \qquad + 2\eta\zeta\left(\frac{d^2u}{dz\,dx}\,\frac{d^2u}{dx\,dy} - \frac{d^2u}{dx^2}\,\frac{d^2u}{dy\,dz}\right) + \quad = 0$$

The "centroid" has the same "criterion function," but with changed sign, as the cubic, viz, it is equal to minus one fourth of the reciprocal of u, with the substitution of $\xi\eta\zeta$ for $\alpha\beta\gamma$, i e, if u be written

$$u \equiv ax^3 + by^3 + cz^3 + \qquad + 6exyz,$$

the criterion function is equal to

$$-(b^2c^2\xi^6 + \qquad)/4,$$

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* Kühne and Chittenden, 'Ueber Albumosen Zeitschr für Biologie,' vol 20

† Nature of Papain &c Journ of Physiol vol 6 p 344

so that the centroid cuts the line at infinity in two real, coincident, or imaginary points, according as two of the three intersections of the cubic with that line are unreal, coincident or real

The discriminant of the centroid is, similarly, equal to minus one fourth of the square of the "Cayleyan" of the cubic, with the same substitutions

A diameter is the locus of mean points of a system of parallel chords, which may be called "its" chords, but through any point pass two chords which have that as mean point Considering the points then on a given diameter, its own chords through those points are all parallel to the polar of its own mean point with respect to the "centroid," which polar is itself a double chord, the other system of chords touch a parabola which is touched by the diameter itself at its own mean point, viz, for that point the diameter is itself the chord of the second system, and the connector of that point with the centre of the "centroid" is a diameter of the parabola To every diameter of the cubic corresponding to a parabola, the envelope of all these parabolas is a quartic curve, while the double chords which are otherwise distinguished as those having their mean points on the "centroid," envelope a second cuspidal quartic

The locus of the mean points of the diameters of the cubic is a second cubic, having a node at the centre of the "centroid," and its asymptotes as its nodal tangents Every diameter cuts this cubic in its own mean point, and the mean points of two other diameters, and this latter pair of points are harmonic conjugates with respect to the first and the point of contact of the diameter with the "centroid"

If the "centroid" is an ellipse its centre is merely an acnode, or conjugate point, on the locus of mean points of diameters

The elliptic cubic—that which has only one real point on the line at infinity—has two real conjugate diameters, *i e*, diameters each of which is the locus of mean points of chords parallel to the other, viz, the asymptotes of the (then hyperbolic) "centroid"

In the case of the parabolic cubic, the "centroid being a parabola, the preceding statements require some modification viz, the envelopes of the double chords and of the parabolas connected each with a diameter of the cubic in the manner above described, degenerate from quartics into parabolas, and on each diameter there lies only the (finite) mean point of one other diameter, this being the mid-point of the segment between the mean point of the diameter itself and its point of contact with the "centroid"

The above are fundamental properties of the diameters of cubics Some of them might have been stated more generally of the second polars of points lying in any right line, but it has been thought proper in this notice to limit the statements to the diameters only, viz., the second polars of points lying on the line at infinity

Presents, May 5, 1887

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May 12, 1887

Professor G G STOKES, D C L, President, in the Chair

The Presents received were laid on the table, and thanks ordered for them

The following Croonian Lecture was delivered —

CROONIAN LECTURE —'On *Parieasaurus bombidens* (Owen), and the Significance of its Affinities to Amphibians, Reptiles, and Mammals' By H G SEELEY, F R S, Professor of Geography in King's College, London Received April 21, 1887

(Abstract)

The author gives a short account of the literature of Parieasaurus, and describes a skeleton in the British Museum, received from the Karoo deposits of South Africa in 1878

The head has the external bones pitted and grooved as in Labyrinthodonts and Crocodiles, and mucous canals are developed between the nares and orbits such as characterise Labyrinthodontia The palate, as evidenced by *Parieasaurus serridens* (Owen), is essentially Anomodont in structure The dentition, with some distinctive features, approximates to that of Dinosaurs and Crocodiles, but though the teeth are in sockets they are cemented to the jaw by bone

The suture between the pre maxillary and maxillary bones is overlapped by a triangular sub nasal bone, and the pre-maxillaries are apparently small, as in Amphibians The maxillary bone has a row of large supra-dental foramina The malar bone is excluded from the alveolar border of the jaw Behind the orbits the bones which form the cheeks are arranged as in Labyrinthodonts, and completely cover the quadrate bone as in Labyrinthodontia and Ichthyopterygia They include the post orbital, post-frontal, squamosal, supra-temporal, and quadrato-jugal, which is of large size The roof bones of the head are the nasal bones, pre frontals, (?) the supra-orbital, frontals, parietals, (?) supra occipital, and epiotic

The occipital condyle, though imperfectly preserved, was single and formed by one bone, which is named basi occipital, and regarded as being an inter central ossification of the vertebral column, which has been received between the exoccipital bones of the Labyrinthodont skull, but which has not penetrated so deeply between them as in typical Anomodonts The nearest approach to this condition of the occipital condyle is seen in Ichthyosaurus The composite character of the occipital condyle in Anomodontia, Chelonia, Rhynchocephalia, Crocodilia, Sauropterygia, Aves, &c , is accordingly regarded as due to the preservation of the original Labyrinthodont exoccipital elements in union with the new basi occipital element And the mammalian return to articulation of the skull by exoccipital condyles is attributed to expansion of the brain which caused the basi occipital bone to enter into the floor of the brain case

The structure of the palate is described, and shown to resemble that of Anomodonts, certain Labyrinthodonts, and the embryonic forms of existing Amphibia in which the para-sphenoid has not been ossified

The lower jaw encloses a long chamber which extends beneath the teeth as in Labyrinthodonts

The dentition is described, and is chiefly remarkable for the uniform character of the teeth, for their distinctive shape, mode of union with the jaw, and replacement by successional teeth, which are developed as in Ichthyosaurs

The skull is compared with the skulls of the types of *P serridens* (Owen) and *P bombidens* (Owen), and the author concludes that there is no satisfactory dental character to distinguish the described species from each other, and rests their differentiation upon the forms, proportions, and structure of the head

Comparison is then made of the cranial characters in which Pareiasaurus resembles other animals, so as to show how far the Labyrinthodont characters are common to Fossil Reptilia, and how far the supposed Dinosaurian characters may be otherwise regarded

The vertebral column comprises 29 vertebræ of which 18 are pre sacral, 2 sacral, and 9 caudal, though a few caudal vertebræ may possibly be missing Crescentic intervertebral wedge bones are developed between most of the vertebræ The ribs all articulate by double heads In the cervical region both facets are on the centrum, and the ribs have long forks for the head and tubercle which are of a Crocodilian character The neural arch is depressed and expanded, and the neural canal small The centrum is comparatively long, and the cervical vertebræ pass into the dorsal series in the same way as in Plesiosaurs

The dorsal vertebræ are nine in number The parapophyses are somewhat elongated and the diapophysis forms a transverse process which is only separated from the post zygapophysial ridge by a notch The neural spine is a short inverted cone nearly as wide at the summit as it is high The neural arch is nearly three times as wide as the centrum is long

The dorsal ribs are in natural contact with the vertebræ they are strong expanded vertically at the proximal ends and directed horizontally outward before they curve backward In the last dorsal the parapophysis is very small The ribs closely resemble those of Crocodiles and Labyrinthodonts There is one lumbar vertebra with long transverse processes

Two vertebræ are anchylosed together in the sacral region but the second does not differ in form from the early caudal, and the pelvis is supported by the first vertebra of the sacrum The sacral rib has a massive development so as to extend along much of the length of the ilium this rib is compared to that of the great Salamanders The same mode of support for the pelvis is found among the Anomodontia The ilium has a mammalian form and position, lying almost entirely in advance of the acetabulum but there is no close correspondence in form with any mammalian genus

The caudal vertebræ rapidly diminish in size, and show no trace of a notochordal condition The neural arch is reduced in width Small caudal ribs were developed

The form of the neural spine in the vertebral column is suggestive of the recent Japanese Salamander, but as a whole the vertebral column has nothing in common with existing Amphibians

The interclavicle and clavicles form an anterior bow, like that seen in Nothosaurians, toward which the clavicular arch of the Ichthyosaurian genus Ophthalmosaurus approximates in the mode of union of the bones The interclavicle is a symmetrical Λ-shaped bone with its limbs diverging and directed backward The scapular arch is very massive, and appears to include coracoid and scapula

The only remains of dorsal armour preserved consist of relatively small and scattered bony scutes

There is no evidence of limb bones

The other remains which have been attributed to Parieasaurus are referred by the author to a new genus named Phocasaurus This is founded upon the ilium originally attributed to Parieasaurus, and the bones hitherto regarded as the anchylosed scapula and coracoid, are now regarded as the anchylosed pubis and ischium of the same individual animal to which the ilium belongs The ilium is intermediate between that of an Ornithosaur and the ilium of a seal. The further detailed discussion of Phocasaurus is reserved One ilium attributed to Parieasaurus is referred to Dicynodon

Parieasaurus is placed in the sub-order Parieasauria, which is grouped under the Anomodontia

On comparison of Parieasaurus with reputed Dinosaurs from South Africa, it is shown that although the teeth of Anthodon resemble those of Acanthopholis, the bones of the post-orbital region of the skull are arranged upon the Labyrinthodont plan which characterises Parieasaurus There is no evidence of the affinities of Tapinocephalus except the sections of the teeth, but since they are hollow and implanted as in Parieasaurus, it is probable that that genus also is not Dinosaurian The only Dinosaurs known from South Africa are the genera Orosaurus and Euskelesaurus described by Professor Huxley in 1866

Concerning the affinities of Parieasaurus, the author interprets the blending of Labyrinthodont and Reptilian characters on the hypothesis that Parieasaurus exhibits a transition from the Amphibia to the Reptilia The animal is regarded as technically a reptile, but as showing both in the skull and vertebral column characters which are typically Labyrinthodont, though they are for the most part unknown among existing Amphibians The Reptile order to which the resemblances come closest is the Anomodontia And since the resemblance in the sacrum and pelvis amounts to absolute coincidence in plan, and in the palate to close approximation, it is inferred that the cranial differences which separate Parieasaurus from the Anomodonts are no more important than those which distinguish the skull of a turtle from the skull of a tortoise, and that with the acquisition of the single basioccipital condyle there came about a gradual loss of the distinctive Labyrinthodont characters But the Labyrinthodontia is a large group including many sub orders, some of which approximate to living Urodeles, others to living Reptilia, and yet others to fossil Reptilia It is urged that the Ichthyopterygia form the most primitive order, derivative from the Labyrinthodont group, having lost the epiotic bones but retaining the post-orbital and supra-temporal with the covered quadrate bone, that the form of vertebra in Ichthyosaurus with its articular tubercles for the rib, reproduces the vertebra of Eosaurus and Anthracosaurus and other Labyrinthodonts, while the dorsal ribs in the two groups are identical Similarly from the structure of Pareiasaurus and its affinities to Mesosaurus, it is argued that the Nothosauria and Plesiosauria are closely related and since they have the long bones ossified as in existing Amphibians with the biconcave vertebræ, double-headed cervical ribs on the centrum (in many genera), with dorsal ribs rising to the neural arch and much in common in the clavicular and pelvic arches it is held to be a legitimate inference that both the Nothosauria and Plesiosauria have undergone, with the acquisition of the basi occipital bone, a loss of Labyrinthodont characters, which has removed from the skull every technical trace of the group from which both of those orders are shown to have been derived, by the Amphibian and Labyrinthodont characters which they have preserved in other parts of the skeleton Similarly it is concluded that the Crocodilia derive their typical characters from the Labyrinthodontia The crocodilian skull preserves the anterior position of the nares and small size of the pre maxillary bones The temporal fossæ are substantially similar and the unossified condition of the post orbital membrane in crocodiles accounts for the absence of the post-orbital and supra temporal bones between the post frontal and squamosal bones above and the jugal and quadrato jugal bones below While if those ossifications were developed in the vacuity behind the eye the quadrate would be completely hidden and the skull would be externally as Labyrinthodont as that of Ichthyosaurus The number of pre sacral vertebræ in crocodiles shows no great divergence from Pareiasaurus, while the mode of articulation of the dorsal ribs in Pareiasaurus although not quite crocodilian, make a nearer approach to the crocodilian type than is seen among other Reptilia and is so similar as to justify the conclusion which the skull suggests—that Crocodilia have been modified from a Labyrinthodont ancestry If this conclusion is admitted for the Crocodilia, it follows for the Dinosauria also, since that group is essentially a parallel variation from the crocodilian type The Proterosauria are already known to show many Labyrinthodont characters, combined with those of reptiles and mammals, and the similar Ornithosaurian pelvis is essentially a variation from that of the Anomodont, and would thus be an inheritance of structures which were of Labyrinthodont origin Hence the Reptilia are to be regarded as related to each other as descendants from a common and varied ancestral type, and, therefore, the orders should be grouped in parallel rather than vertical or successive relation to each other

The mammalian characters of the pelvis and sacrum of Pareiasaurus and the other Anomodonts are quite as striking as the Avian characters of certain Dinosaurs, and of the same kind of importance as evidence of affinity If the community of structure of Iguanodonts and Birds is held to establish a common origin for both groups, then the community of structure with mammals which appears in the pelvis in Pareiasaurus and is variously developed in other parts of the skeleton in many allied genera of Anomodontia and Theriodontia, must similarly be held to establish a common origin for these mammalian and reptilian structures by inheritance from amphibian ancestors

The Society adjourned over Ascension Day to Thursday, May 26th

Presents May 12, 1887

Transactions

Christiania —Videnskabs Selskab Forhandlinger Aar 1886 8vo *Christiania* 1887 — The Society

Gottingen —Konigl Gesellschaft der Wissenschaften Abhandlungen Band XXXIII 4to *Gottingen* 1886 Nachrichten Jahr 1886 Nro 1 20 8vo *Gottingen* 1886 — The Society

London —Entomological Society Transactions 1887 Part 1 8vo *London* — The Society

Institution of Mechanical Engineers Proceedings 1887 No 1 8vo *London* — The Institution

Odontological Society Transactions Vol XIX No 6 8vo *London* 1887 — The Society

Royal Asiatic Society Journal Vol XIX Part 2 8vo *London* 1887 — The Society

Royal Institution Proceedings Vol XI Part 3 8vo *London* 1887 The Work of the Imperial Institute Address by Sir F Abel F R S 8vo *London* 1887, List of Members, 1886 8vo *London* — The Institution

Royal Medical and Chirurgical Society Proceedings Vol II No 5 8vo *London* 1887 President s Address, 1887 8vo *London*, Catalogue of the Library Suppl V 8vo *London* 1887 — The Society

Vienna —Anthropologische Gesellschaft Mittheilungen Band XVI Hefte 3-4 4to *Wien* 1886 — The Society

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Boehmer (G H) Norsk Naval Architecture 8vo [*Washington*] 1887 — The Author

Bowman (F H) Colonial and Indian Exhibition Reports Wools 8vo [*London*] 1887 — The Author

Brunton (T Lauder), F R S A Text Book of Pharmacology, Therapeutics, and Materia Medica Third Edition 8vo *London* 1887 — The Author

Dawson (G M) Notes to accompany a Geological Map of the Northern Portion of the Dominion of Canada 8vo *Montreal* 1887 On certain Borings in Manitoba and the North West Territory 4to *Montreal* 1887 The Author

Dawson (Sir J W) F R S On the Fossil Plants of the Laramie Formation of Canada 4to [*Montreal*] 1886 The Author

Dubois (A) Description de Deux Nouvelles Espèces d'Oiseaux 8vo [*Bruxelles*] 1887 The Author

Hughes (F J) Supplement to Harmonies of Tones and Colours developed by Evolution 4to *London* 1885 The Author

Marsh (O C) American Jurassic Mammals 8vo [*New Haven*] 1887 The Author

Martone (M) Sopra un Problema di Analisi Indeterminata [Two copies] 8vo *Catania* 1887 The Author

May 26, 1887

Professor G G STOKES D C L, President in the Chair

The Presents received were laid on the table, and thanks ordered for them

Professor Archibald Liversidge (elected 1882) was admitted into the Society

The following Papers were read —

I THE BAKERIAN LECTURE —'On the Dissociation of some Gases by the Electric Discharge' By J J THOMSON, M A, F R S, Fellow of Trinity College and Cavendish Professor of Experimental Physics in the University of Cambridge Received May 26, 1887

(Abstract)

The gases considered are iodine, bromine chlorine and nitrogen tetroxide The effects of the spark on iodine and bromine were investigated in two ways In the first method the iodine was placed in a tube from which the air had been exhausted and which was furnished with a gauge which served to measure the changes of pressure in the tube The liquid in the manometer was sulphuric acid, and in order to avoid any disturbance due to the absorption of the iodine vapour by this substance, the discharge tube was doubled so that the iodine vapour was symmetrically placed with reference to the sulphuric acid. The system was then placed in an oil bath and maintained at a temperature which varied in different experiments from about 200° to 230.

On sparking through such a tube with an induction coil giving a spark about 3 inches long in air, the pressure rapidly increases at first but the rate of increase gradually diminishes and the pressure finally becomes steady. On stopping the coil by far the greater part of this increase is permanent or at any rate lasts for several hours. It is not due to the decomposition of the vapour from the sulphuric acid in the gauge, for it does not occur if there is no iodine in the gauge or if the iodine is replaced by bromine. This increase of pressure can be produced by the silent discharge as well as by ordinary sparking. In order to simplify the conditions as much as possible I had an arrangement made by which instead of determining the increase of pressure by the sulphuric acid gauge the vapour density of the iodine after sparking could be measured. In this arrangement the iodine was never near any sulphuric acid.

The result of these determinations is shown in the following table, and it is seen that the results confirm those obtained by the first method.

Unsparked iodine—

Pressure	Temperature	(H = 1) Vapour density
440	215	137
420	214	130

Sparked iodine—

Pressure	Temperature	Vapour density
618	220	110
420	216	115
166	214	84
170	232	86

In the last experiment the vapour density was determined 24 hours after the sparking.

These figures point to very considerable dissociation of the iodine, in fact the dissociation produced by the spark at 214° is as much as that produced by Victor Meyer at the temperature 1570° C.

The appearance of the dissociated iodine is not greatly different from that of the unsparked, its colour, however, is I think a little lighter and not so uniform. I was not able to detect any change in the absorption spectrum produced by the sparking. The electric strength of the sparked gas was however less than that of the unsparked.

Bromine.

When the experiment with the pressure gauge is made with bromine instead of iodine, it is found that there is a considerable increase of pressure produced by the passage of the spark, but that this disappears almost as soon as the sparking, and on determining the vapour-density of the sparked and unsparked bromine it is found that they are identical. It seems most probable that the difference between bromine and iodine is not that the bromine is not dissociated by the spark, but that the atoms combine very much more quickly than the iodine atoms The vapour-density determinations showed that bromine vapour is dissociated if it is heated for a long time at a low pressure, even though the temperature is not very high.

The results of these determinations are given in the following table:—

Pressure	Temperature	Density	Remarks
473	111	80	
466	106	81	
430	101	80	
602	116	79	
543	89	81·7	In bath for 24 hours
315·5	105	73	
235	109	77	*Sparked*
230	100	66·5	In bath for 4 hours
165	90	77	Only a short time in bath
390	111	70	In bath for 7 hours

These experiments show that it takes a long time for bromine to reach a state of equilibrium, and that for the experiments on the vapour-density, the gas should be maintained at a constant temperature for some time before the experiments are made.

Experiments on chlorine and nitrogen tetroxide are also described in the paper.

II. "On the Supposed 'New Force' of M. J. Thore."* By WILLIAM CROOKES, F.R.S., Pres.C.S. Received May 5, 1887.

(Abstract.)

The author commences by quoting the description of some apparatus and experiments which have led M. Thore to suspect the existence of a new force inherent in the human organism. M. Thore suspends a small cylinder of ivory by a fibre of cocoon silk, forming a small pendulum, which hangs freely over the centre of a table. The cylinder having become motionless, M. Thore brings a second cylinder, called the "pillar," about a millimetre from the first cylinder, when the latter begins to rotate clockwise if the pillar is on the left, and counter-clockwise if the pillar is on the right of the cylinder. The observer is supposed to face the cylinder and pillar. M. Thore says that the rotation is independent of the nature of the cylinders, of their mass, or the dimensions of the pillar; light, heat, electricity, magnetism, gravity, and air currents, he says, are also inadequate to explain the phenomena.

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* 'Une Nouvelle Force?' Par J. Thore. Dax, 1887.

The author has repeated M. Thore's experiments in apparatus shown in the accompanying figure. It consists of a glass case, A, B, C, D, E, F, 6½ inches square and 7 inches high, with a rising glass window, A, B, G, H, in front and similar windows at the sides. The top is of card, in the centre of which is a small hole. The cylinder, I, is suspended in the middle of the case by a very fine cocoon silk fibre, 5 feet long, surrounded by a card tube J, attached to the top of the glass box. K is a second cylinder attached to a support, L, M, by a ball and socket joint for convenience of adjustment. The support, M, projects outside the case to admit of the *pillar* being brought close to the *cylinder* and transposed from one side to the other, &c. N is a cord attached to the front glass window, weighted at the end and passing over a pulley for convenience of raising and lowering the glass.

Ivory, ebonite glass and metal have been used for the cylinders and for the pillars. The pillars have also been made square, round, and wedge shaped in section, and the surfaces have been bright and lamp blacked. The mode of experimentation is the following:—The cylinder being at rest the observer sits down in front of the apparatus with his face 8 inches from the cylinder and pillar, taking precautions to keep the breath as much as possible away from cylinder and pillar. The pillar is always placed on the right of the cylinder. On raising the front glass the cylinder commences to rotate in the opposite direction to the hands of a clock, the side nearest the observer moving to the right.*

In other experiments a flask of boiling water a candle, and a hot platinum wire have been used as the source of radiation. The results of more than fifty experiments are given in tables showing the material of the pillar, the maximum speed of one revolution, the number of revolutions, and the exciting agent. Experiments tried with an ascending current of air of different degrees of intensity in front of the apparatus prove that air currents are inoperative in producing the action.

The results leave little doubt that the action is one of radiation from the face or other warm body in front of the apparatus, and that there is nothing special in the human organism beyond the heat it radiates to produce rotation of the cylinder.

Radiant heat (and in less degree light) falling on the lampblacked surfaces is absorbed, and increases the surface temperature. There are two ways in which this increase of temperature may act:—

1. It may produce a current of warm air, rising in front of the surfaces of the moving body; to replace this, cold air will come in from all sides, and striking against the delicately suspended cylinder cause it to rotate. If, however, the source of heat is of considerable surface, such as the face or a Winchester quart bottle full of warm water, it is difficult to imagine that there will be much tendency to rotate in one direction rather than in the other.

2. An increased surface temperature of the cylinder and pillar may produce an increase of molecular pressure between the two bodies,

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* This is called the *negative* direction and when the rotation is clockwise it is called *positive*.

and thus give rise to motion, after the manner of the radiometer. In this, as in the former case, the movement should be in the opposite direction to what it is in reality, as it would be produced by mutual repulsion acting between the sides nearest the source of heat.

It seemed likely that information, decisive as regards one or other of these two theories, might be gained by suspending the cylinder in a glass tube attached to a Sprengel pump, and taking observations at different degrees of exhaustion.

In experiments tried by the author in 1875[*] the noteworthy fact was ascertained, that two bodies of different temperature attracted each other at normal atmospheric pressure; the attraction rose as the pressure diminished, until, at a tension of 1·15 mm., it was nearly four times what it was in dense air. Above this exhaustion the attraction suddenly dropped and changed to repulsion, which at the best vacuum obtained was nearly thirteen times stronger than the attraction in air.

Two forms of apparatus are described by the author, wherewith experiments were tried during exhaustion, and an exact parallelism was established between the attraction or repulsion of the cylinder by a hot platinum spiral, and the positive or negative rotations of the cylinder under the influence of a warm body brought near.

The two phenomena run absolutely in parallel lines; when there is attraction negative rotation is also produced; when the exhaustion is such that the attraction is *nil*, the rotation is *nil* also; when the attraction changes to repulsion the rotation changes from negative to positive; and when the vacuum is very good, so that the repulsion between the two heated bodies is at its maximum, then also the positive rotation is the strongest. It is impossible to resist the conclusion that the two sets of phenomena are due to the same cause, and that as air currents did not produce the attractions observed in the 1875 experiments, so likewise are they equally inoperative in giving rise to the present rotations of the suspended cylinder.

If the rotation is produced by a reaction between the suspended and fixed body, it follows that were both free to move each would rotate, but in opposite directions. To test this, another apparatus was made, having two delicately suspended cylinders, 1 mm. apart, in a glass tube capable of being exhausted. In a table the results of twenty-two experiments are described, observations having been taken at intervals during exhaustion. Down to 14 mm. pressure the two cylinders rotate negatively (*i.e.*, the right hand cylinder rotates clockwise, and the left hand cylinder counter-clockwise). Between 14 and 3 mm. there is no rotation, and below 3 mm. the rotation is positive, the movement at an exhaustion of 0·0495 mm. being five times as strong as it was originally.

[*] 'Phil. Trans.,' 1875, Part II (pp. 528–532).

The motive force producing these rotations is, at high exhaustions, the molecular impacts between adjacent surfaces of the suspended cylinders excited by the radiation falling on them from the hot water, hot spiral or a candle (which is equally effective). But what produces the negative rotation at ordinary atmospheric pressure? *Air currents* are the obvious explanation, but there are grave reasons for believing this explanation inadequate. In the first place, actual air currents when tried do not produce the desired result. Secondly, it is most logical to assume that as the present set of experiments are strictly parallel with those tried in 1875, and as in each case the results at high exhaustions are due to molecular bombardment, so also must the similar results at low exhaustions be due to the same cause.

Finally, twenty-one experiments in the form of a table are described, in which an apparatus was employed specially designed to eliminate the interfering action of air currents, and submit the molecular bombardment theory to crucial experiments. The results are considered by the author as conclusive in favour of this explanation.

Addendum, May 24, 1887.

I sent M. Thore a detailed account of my experiments, asking him to favour me with any comments or remarks he might wish to make. I have just received a long communication, partly printed and part in MS., in which he describes many fresh experiments, and adduces arguments to show that my dynamical explanation is not sufficient to account for more than a few of the facts he describes, and saying that he 'persists in still believing that this force emanates from the observer, or else that the observer is the indispensable intermediary for its manifestation.'

The experiments are numerous and are devised with great ingenuity. It is impossible in the space of a brief abstract to do more than refer to a few of the principal facts here brought forward. M. Thore commences by objecting to my having experimented in an enclosed space, saying that he always operates in free air. He thinks that enclosure may almost or quite suppress his force. To this I can reply that I have myself verified nearly all M. Thore's facts of rotation (including those just now communicated), when working in the free air of a large room, and it was only when I found the delicacy of the observations was impeded by draughts and currents that I put screens round the apparatus. I have not found glass screens interfere materially with any of the rotations. M. Thore now says that it is necessary to hold the pillar or the exciting body in contact with the hand during the whole duration of the experiment. I was not aware that importance was attached to this point, but I have since repeated many of my former observations, holding the pillar in the hand The results are certainly stronger, but the extra heat imparted to the apparatus is in my opinion sufficient to account for this M Thore brings forward many new and ingeniously devised experiments to prove that heat cannot be considered the cause of the movement He exposes the instrument to the full sun and then brings it into a cool dark room, he suspends it over boiling water, he places a large block of ice between the cylinder and the observer, he similarly interposes metallic vessels full of boiling water between the cylinder and observer (the observer not moving from his place in front), and he tries the experiment in a hot chamber alternately moist and dry, without finding the regularity of the movements interfered with I have tried most of these, and obtained results corroborating M Thore's, but I have also tried the experiment of quietly bringing near to the stationary cylinder a bottle of hot water and observing the movement from a safe distance through a telescope, and I find that the hot bottle is able to effect rotation as well as the observer

Among the curious observations mentioned by M Thore is this — Placing the pillar in front of the cylinder (between it and the observer), if the pillar is held with the right hand the movement is clockwise, and if the left hand is used the rotation is counter clockwise The right hand is stronger in its effects than the left hand in the proportion of 2 to 1

M Thore has given in addition a large number of curious and interesting observations, using two, three, and more movable cylinders and recording their movements under a great variety of circumstances I admit I do not see at once how all these are to be explained on the molecular bombardment theory But this theory has not yet explained all the anomalous results I have recorded in my papers on "Repulsion resulting from Radiation,' although I believe it capable of doing so; and I therefore think that it is not necessary to call upon a new force to explain any of M Thore's results which radiation does not yet seem able to account for

The Society adjourned over the Whitsuntide Recess to Thursday, June 9th

Presents, May 26, 1887

Transactions

Buckhurst Hill —Essex Field Club The Essex Naturalist No 4 8vo. *Buckhurst Hill* 1887 The Club

Leeds —Naturalists' Club Transactions 1886 8vo *Leeds* 1886 The Club

Transactions (*continued*)

London.—East India Association. Journal. Vol. XIX. No. 3. 8vo. *London* 1887. The Association.

University. Calendar. 1887-8. 8vo. *London* 1887. The University.

Rio de Janeiro.—Museu Nacional. Archivos. Vol. VI. 4to. *Rio de Janeiro* 1885. The Museum.

St. Petersburg.—Académie Impériale des Sciences. Mémoires. Tome XXXIV. Nos. 12, 13. Tome XXXV. No. 1. 4to. *St. Pétersbourg* 1886-7. The Academy.

Venice.—Reale Istituto Veneto. Atti. Tomo III. Disp. 10. Tomo IV. Disp. 1-10 and Appendice. Tomo V. Disp. 1. 8vo. *Venezia* 1884-7. The Institute.

Vienna.—K. K. Geographische Gesellschaft. Mittheilungen. 1876. 8vo. *Wien* 1876. The Society.

Watford.—Hertfordshire Natural History Society. Transactions. Vol. IV. Part 5. 8vo. *London* 1887. The Society.

Journal.

Ateneo (L.) Veneto. Rivista mensile di Scienze Lettere ed Arti. Serie X. Vol. I. No. 1—6. Vol. II. No. 1—6. 8vo. *Venezia* 1885-6. The R. Istituto Veneto.

Abel (Sir F.) F.R.S. The Work of the Imperial Institute.—An Address. 8vo. *London* 1887. The Author.

Albrecht (Dr. P.) Verlauft der Nervenstrom in geschlossener Strombahn? 8vo. *Erlangen* 1887. With two other excerpts in 8vo. The Author.

Bagnoli (U.) Teorie Fondamentali dell' Elettricità. Sm. 8vo. *Milano* 1887. The Author.

Bowman (F. H.) On some Variations in the Structure of Wool and other allied Fibres. 8vo. *Edinburgh* [1886]. The Author.

Currier (A. F.) Some Considerations concerning Cancer of the Uterus, especially its Palliative Treatment in its Later Stages. Sm. 8vo. *New York* 1887. The Author.

Fleming (S.) Documents in reference to the General Adoption of the Twenty-four Hour Notation on the Railways of America. 8vo. *Ottawa* 1887. Time Reckoning for the Twentieth Century. 4to. *Montreal* 1886. The Author.

Folmer (N.) Rebus over het Licht, het Beeld en de Prismatische Kleuren, in 500 Figuren op 60 Platen. Folio and 8vo. *Groningen* [1887]. The Author.

Hill (S. A.) On Solar Thermometer Observations at Allahabad. 8vo. *Calcutta* 1887. The Author.

June 9, 1887.

The Annual Meeting for the Election of Fellows was held this day.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Dr. J. H. Gladstone and Mr. G. J. Symons were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society:—

Buchanan, John Young, M.A.
Cash, John Theodore, M.D.
Douglass, Sir James Nicholas, M.I.C.E.
Ewing, Prof. James Alfred, B.Sc.
Forbes, Prof. George, M.A.
Gowers, William Richard, M.D.
Kennedy, Prof. Alexander B. W., M.I.C.E.
King, George, M.B.
Kirk, Sir John, M.D.
Lodge, Prof. Oliver Joseph, D.Sc.
Milne, Prof. John, F.G.S.
Pickard-Cambridge, Rev. Octavius, M.A.
Snelus, George James, F.C.S.
Walsingham, Thomas, Lord.
Whitaker, William, B.A.

Thanks were given to the Scrutators.

June 16, 1887.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Right Hon. the Earl of Rosebery (elected 1886), Mr. H. C. Russell (elected 1886), Mr. John Young Buchanan, Dr. John Theodore Cash, Sir James Nicholas Douglass, Prof. James Alfred Ewing, Prof. George Forbes, Dr. William Richard Gowers, Prof. Alexander B. W. Kennedy, Sir John Kirk, Mr. George James Snelus, and Lord Walsingham, were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Structure of the Mucilage Cells of *Blechnum occidentale* (L.) and *Osmunda regalis* (L.)" By TOKUTARO ITO, F.L.S., and WALTER GARDINER, M.A. Communicated by Professor M. FOSTER, Sec. R.S. Received May 31, 1887.

The growing point of many ferns is found to be covered with a slimy mucilage which arises from hairs situated on the paleæ and the leaves, or where paleæ are absent on the leaves only. This mucilaginous secretion serves a most important physiological function, in that it readily takes up and retains water, and thus keeps the young bud moist, and at the same time tends to prevent too excessive transpiration. The cells which secrete the mucilage are large and swollen, and the secretion escapes by the rupturing of the cell wall. We investigated two cases of mucilaginous secretion, viz., *Blechnum occidentale* (L.), where in each hair only the terminal cell is glandular, and *Osmunda regalis* (L.), where usually all the cells of the hair are equally endowed with secretory function. We find that the mucilage arises from the protoplasm only and not from the cell wall, and that the whole process is distinctly intraprotoplasmic. The structure of a mature gland is wonderfully like that of the secretory animal cells investigated by Langley,* and indeed the very words used by him in the description of certain of the secretory cells will quite well apply to the particular glands investigated by us, for we also find that "in the mature cells the cell substance is composed of (*a*) a framework of living substance or protoplasm connected at the periphery with a thin continuous layer of modified protoplasm" (our ectoplasm), and that "within the meshes of the framework are enclosed two chemical substances at least, viz. (*b*), a hyaline substance in contact with the framework, and (*c*) spherical granules which are embedded in the hyaline substance." In our case we have also to add, that the whole cell is enclosed in a cell wall. We find, in other words, that in the glandular cells, investigated by us, mucilage is secreted in the form of drops, and that each drop is further differentiated with a ground substance (gum mucilage) in which are embedded numerous spherical droplets (gum).

The mature cells which we have described are quite full of the secretion, so that the vacuole containing the cell sap has become completely obliterated. This is occasioned mainly by the voluminous character of the secretion, which takes up water and becomes very bulky. The young glands, however, display the usual structure of young cells, each containing a nucleus, plastids (which in the case of *Osmunda* form numerous starch grains), and a vacuole. Secretion commences by the breaking down of a portion of the innermost layers of the endoplasm at a number of contiguous but isolated areas The result of these katabolic changes in the protoplasm is the formation of small but rapidly growing mucilage drops The first formation occurs just beneath the free surface, and takes place equally around the whole cell cavity and the phenomenon steadily continues from within outwards, producing new drops basipetally, and immediately beneath those already formed until the whole of the endoplasm together with the substance of the plastids (or starch grains) have taken part in the process and the cell is now full of isolated drops each enclosed by a portion of the delicate protoplasmic framework which still remains

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* Langley, 'Cambridge Phil. Soc. Proc.,' vol. 5, p. 25.

A remarkable sequence of changes occurs in the drops themselves At their first formation they are watery and by no means well defined By the use of osmic acid it can then be shown that they contain no tannin They shortly become denser and at this stage tannin appears equally distributed throughout their structure And now in the drops themselves a delicate reticulation may be observed which finally gives way to the appearance of numerous minute and brightly shining droplets all separate and distinct disseminated through the substance of the drop just as the drops themselves are disseminated through the substance of the protoplasm Reactions show that the ground substance of the drops is of the nature of a gummy mucilage, while the drops consist of pure gum Our observations make us disposed to believe that during secretion the protoplasm gives rise to a gummy mucilage and the latter undergoes further differentiation into a ground substance which still retains its mucilaginous character, and into a gummy substance (a product probably of maximum chemical change) which is present as a number of isolated spherical droplets In the light of these remarks the structure presented by the mature cell becomes more clear

In the case of many animal glands, *e g*, serous and mucous salivary glands, Langley concludes that the protoplasm forms the hyaline substance, and then out of this manufactures the granules, which during secretion are turned out of the cell and give rise to the particular substance which the gland secretes The state of active secretion is followed by a resting period during which the protoplasm grows, forms new hyaline substance, and this again produces new granules We believe that a series of changes essentially similar in character occurs in plant cells also Usually speaking, plant cells are incapable of such active and repeated secretion as occurs on those of animals, and in many cases, *e g*, *Blechnum* and *Osmunda*, the secretion changes occur in the cell once and for all, and at their termination the cell dies In other instances, however, *e g*, the glands of *Dionæa*, it appears exceedingly probable that the phenomena which accompany the repeated secretion are quite similar to those which happen in so many animal glands.

The various changes which accompany mucilaginous secretion are not shown by *Blechnum occidentale* In *Osmunda* the drops are much less defined, and, although more numerous, are smaller. The changes which occur in the drops were observed in *Blechnum occidentale*. In *Osmunda* we did not succeed in following them; but since the two glands practically present the same structure in the mature cells, we are led to infer that the various processes are similar in both.

The secretion consisting of the mucilage drops and the disorganised protoplasmic framework escapes by the rupturing of the wall, and the disintegrated nucleus and the endoplasm are the only structures left in the cell.

In *Osmunda* the transverse walls are callussed on both sides, and the whole system (wall and callus plate) is obviously perforated by fine holes, which in the functional cell are filled by delicate strands of protoplasm. These establish a direct continuity between the protoplasmic contents of the various cells of the hair.

We believe that in their main features the phenomena attending the formation of the secretion are such as are very widespread, and limited neither to the ferns nor to the particular case of secretion of mucilage.

II. "On Rabies." By G. F. DOWDESWELL, M.A. Communicated by Prof. VICTOR HORSLEY, F.R.S. (From the Laboratory of the Brown Institution.) Received May 9, 1887.

(Abstract.)

In this investigation, commenced early in 1885 during the outbreak of rabies in London, the first experiments, made by subcutaneous inoculations with the saliva of rabid street dogs, all failed to produce infection.

Subsequently, adopting the methods described by M. Pasteur, I found—

1. That the virus of rabies and hydrophobia resides in the cerebrospinal substance and in the peripheral nerves, and is not confined to the salivary glands, as hitherto supposed.

2. That by inoculation of this substance upon the brain of another animal, by trephining, infection follows much more quickly and certainly than by subcutaneous inoculation.

3. That rabies, however produced, in both dogs and rabbits, is essentially a paralytic affection, the same disease in both animals, and

that there is no constant distinction between the so-termed "dumb" and "furious" rabies.

4. That the initial virulence of street rabies is usually increased, and becomes remarkably constant, by passing through a series of rabbits.

5. That the activity of the virus is shown by the duration of the incubation period, to which it is inversely proportionate.

6. That the tissues of an infected animal do not themselves become infective till towards the end of the incubation period.

7. That of a large number of drugs which were tried, both germicides and those acting specifically upon the cerebro-spinal system, none materially modify the action of the virus in the rabbit.

8. That by a series of subcutaneous inoculations with virus treated by the methods of M. Pasteur, immunity, even against subsequent infection, cannot be conferred upon the rabbit; and that the extreme and unexpected constitutional refractoriness of the dog to infection with rabies, by any method of inoculation—as I have found it in the limited number of experiments I have been able to perform with this animal—renders it extremely difficult to determine the effect of such remedial or prophylactic measures in it; and that it is by the statistics of the treatment alone that their effect with man can be decided; but that judging from the results of the experiments of others, the principle of the method as affirmed by M. Pasteur appears to be established, though unquestionably the "rapid" or "intensive" treatment, as I have found, is liable to produce infection.

III. "On the Tubercular Swellings on the Roots of *Vicia Faba*." By H. MARSHALL WARD, M.A., F.L.S., Fellow of Christ's College, Cambridge, Professor of Botany in the Forestry School, Royal Indian College, Cooper's Hill. Communicated by Prof. M. FOSTER, Sec. R.S. Received May 29, 1887.

(Abstract.)

In this paper the author gives a detailed account of his investigations, of which a preliminary note appeared at p. 331. The following are the main conclusions:—

The tubercles always contain a fungus, allied to the Ustilagineæ, which enters the root by way of the root hairs. The ultimate branches of the hyphæ in the cells of the tubercle bud off minute bodies (gemmules), which are afterwards scattered in the soil. This process resembles the budding discovered in Ustilagineæ by Brefeld. By means of cultures and observations the author shows that the infection from the soil is probably due to these minute gemmules acting as spores.

IV. "The Electromotive Properties of the Electrical Organ of *Torpedo marmorata*." By FRANCIS GOTCH, B.A., B.Sc. London, M.A. Oxon. Communicated by Professor BURDON SANDERSON, F.R.S. Received May 5, 1887.

(Abstract.)

After an introduction, in which the author sets forth the present state of knowledge with reference to the electromotive properties of the electrical organ of Torpedo, he gives an account of his own experimental investigations in three sections.

The first section relates to the nature of the changes produced in the electrical organ by mechanical injury and by heat, and the relation of these changes to those which manifest themselves under similar conditions in muscle and nerve, a subject which has not hitherto been inquired into.

In the second, the duration and the character of the response of the electrical organ to stimulation of its nerve are investigated for the first time by means of the rheotome and galvanometer.

In the experiments which are recorded in the third section, the author has entered on the examination of the after-effects which are produced in the organ by the passage through it of voltaic or induction currents, a subject which has been recently investigated by du Bois-Reymond.

The author is led by his experiments to believe that the physiological effects produced in the organ by injury, by the passage of currents, and by the stimulation of the electrical nerve, are, notwithstanding that they differ so widely from each other in distribution, duration, and intensity, all phenomena of excitation.

V. "On Thermal Radiation in Absolute Measure." By J. T. BOTTOMLEY, M.A. Communicated by Sir W. THOMSON, Knt., F.R.S. Received April 23, 1887.

(Abstract.)

The investigation, of which a detailed account is given in the paper, was commenced in 1883, and some preliminary results were communicated to the Royal Society in June, 1884.

The radiating body used up to the present time has been a metallic wire;* and the general method of experimenting consists in keeping

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* I propose, however, as soon as may be, to repeat and extend the experiment of D. Macfarlane ('Roy. Soc. Proc.' vol. 20, 1872) on radiation from metallic globes.

the wire heated by an electric current, and determining in absolute measure the electric energy necessary for this purpose. This energy is lost in radiation and in conduction at the ends, but chiefly in radiation. The wire is contained in a long copper tube, blackened inside, and kept cool by a water jacket; and the surface of the wire may be bright and polished, or may be modified by being coated with lamp-black, platinum-black, oxide of copper, or some other material Polished wires have been chiefly used hitherto, but arrangements for comparison of wires with surfaces differently prepared are described in the paper.

Two methods of determining the electric energy have been used. One consists in measuring the electric current and the difference of potentials between chosen points in the radiation-wire; the other in measuring the current and determining simultaneously the electric resistance, by means of a Wheatstone bridge, modified to suit the necessities of the case.

A knowledge of the resistance of the radiation-wire gives also its temperature, by means of separate determinations (described in the paper) of the law of alteration of electric resistance with temperature. The temperatures of the radiation-wire and of the envelope are all referred to the air thermometer.

In order to vary the air-pressure surrounding the radiation-wire, and thus obtain data for the purpose of eliminating the heat-carrying properties of the air or other gas, the copper tube is connected, in a manner described in the paper, with a five-fall Sprengel pump; and the pressures in the extreme vacuums are measured by the M'Leod gauge.

The results of the investigation, so far as it has gone, are shown in a series of tables and curves.

A long and very complete series of determinations has been made of the radiation at various given constant pressures, but at different gradually increasing temperatures.

By means of curves of this kind, showing radiation in extreme vacuum, a comparison may be made between the results of experiment and the results calculated from Stefan's well-known fourth power law. The experimental results do not appear to give any support to that law.

Several series of determinations have been made at different constant temperatures, the pressure being continuously diminished. This mode of experimenting, by far the most appropriate to the purpose in hand, has only recently become convenient to the author, as it requires a special suitable current galvanometer. Further experiments are to be carried out with this method.

In the meantime it may be said that on continuously diminishing, by means of the Sprengel pump, the air surrounding the wire, a point has been reached where further exhaustion does not affect the radiation observed. In this way a condition seems to be reached asymptotically, in which the radiation is independent of anything removable by the Sprengel pump. The value of the radiation found is, for the particular bright platinum wire used:—

At 408° C. ...... $378\cdot8\times10^{-4}$ gram water centigrade units per square centim. per sec.

„ 505° C. ...... $726\cdot1\times10^{-4}$ gram water centigrade units per square centim. per sec.

the temperature of the envelope being about 15° C.

Comparatively little has been done up to the present as to radiation from the same body with the surface in different conditions. The important results of Mr. Mortimer Evans, 'Roy. Soc. Proc.,' 1886, as to the energy required to maintain a given candle power in incandescent lamps, with dull and with polished filaments, have been confirmed. It is proposed to carry out further experiments on the influence of the surface of radiating bodies.

VI. "On Figures of Equilibrium of Rotating Masses of Fluid." By G. H. Darwin, M.A., LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge. Received April 28, 1887.

(Abstract.)

The intention of this paper is, first, to investigate the forms which two masses of fluid assume when they revolve in close proximity about one another, without relative motion of their parts; and secondly, to obtain a representation of the single form of equilibrium which must exist when the two masses approach so near to one another as just to coalesce into a single mass.

When the two masses are far apart the solution of the problem is simply that of the equilibrium theory of the tides. Each mass may, as far as the action on the other is concerned, be treated as spherical. When they are brought nearer to one another this approximation ceases to be sufficient, and the departure from sphericity of each mass begins to exercise a sensible deforming influence on the other.

The actual figure assumed by either mass may be regarded as a deformation due to the influence of the other considered as a sphere, on which is superposed the sum of an infinite series of deformations of each due to the deformation of the other and of itself.

But each mass is deformed, not only by the tidal action of the other, but also by its own rotation about an axis perpendicular to its

orbit. The departure from sphericity of either body due to rotation also exercises an influence on the other and on itself, and thus there arises another infinite series of deformations.

It is shown in the paper how the summations of these two kinds of reflected influences are to be made, by means of the solution of certain linear equations for finding three sets of coefficients.

The first set of coefficients are augmenting factors, by which the tide of each order of harmonics is to be raised above the value which it would have if the perturbing mass were spherical. The second set correspond to one part of the rotational effect, and belong to terms of exactly the same form as the tidal terms, with which they ultimately fuse. The third set correspond to the rest of the rotational effect, and appertain to a different class of deformation, which are in fact sectorial harmonics of different orders. The term of the second order represents the ellipticity of the mass due to rotation, augmented, however, by mutual influence. All the terms of this class, except the second, are very small; their existence is, however, interesting.

From the consideration that the repulsion due to centrifugal force shall exactly balance the attraction between the two masses, the angular velocity of the system is found. It is greater than would be the case if the masses were spherical.

The theory here sketched is applied in the paper numerically, and illustrated graphically in several cases.

When the masses are equal to one another they are found to be shaped like flattened eggs, and the two small ends face one another. Two figures are given, in one of which the small ends nearly touch, and in the other where they actually cross. In the latter case, as two portions of matter cannot occupy the same space, the reality must consist of a single mass of fluid consisting of two bulbs joined by a neck, somewhat like a dumb-bell. In the figure conjectural lines are inserted to show how the overlapping of the masses must be replaced by the neck of fluid.

A comparison is also made between the Jacobian ellipsoid of equilibrium with three unequal axes and the dumb-bell. It appears that with the same moment of momentum the angular velocity is nearly the same in the two figures, but the kinetic energy is a little less in the dumb-bell. The intrinsic energy of the dumb-bell is, however, greater than that of the ellipsoid, so that the total energy of the dumb-bell is slightly greater than that of the ellipsoid.

Sir William Thomson has remarked on the "gap between the unstable Jacobian ellipsoid . . . . . and the case of the smallest moment of momentum consistent with stability in two equal detached portions." "The consideration," he says, "of how to fill up this gap with intermediate figures is a most attractive question, towards answering which we at present offer no contribution."* This paper is intended to be such a contribution, although an imperfect one.

M. Poincaré has made an admirable investigation of the forms of equilibrium of a single rotating mass of fluid, and has specially considered the stability of Jacobi's ellipsoid.† He has shown by a difficult analytical process, that when the ellipsoid is moderately elongated, instability sets in by a furrowing of the ellipsoid along a line which lies in a plane perpendicular to the longest axis. It is, however, extremely remarkable that the furrow is not symmetrical with respect to the two ends, and there thus appears to be a tendency to form a dumb-bell with unequal bulbs.

M. Poincaré's work seemed so important that, although the figures above referred to were already drawn a year ago, this paper was kept back in order that an endeavour might be made to apply the principles enounced by him, concerning the stability of such systems. The attempt, which proved abortive on account of the imperfection of approximation of spherical harmonic analysis, is given in the appendix to the paper, because, notwithstanding its failure, it presents features of interest.

The calculations in this paper being made by means of spherical harmonic analysis, it is necessary to consider whether this approximate method has not been pushed too far in the computation of figures of equilibrium which depart considerably from spheres. A rough criterion of the applicability of the analysis is derived from a comparison between the two values of the ellipticity of an isolated revolutional ellipsoid of equilibrium as derived from the rigorous formula and from spherical harmonic analysis As judged by this criterion, which is necessarily in some respects too severe, the figures drawn appear to present a fair approximation to accuracy.

Since, as above stated, the rigorous method of discussing the stability of the system fails, certain considerations are adduced which bear on the conditions under which there is a form of equilibrium consisting of two fluid masses in close proximity, and it appears that there cannot be such a form, unless the smaller of the two masses exceeds about one-thirtieth of the larger. It seemed therefore worth while to find to what results the analysis would lead when two masses, one of which is 27 times as great as the other, are brought close together. As judged by this criterion the computed result must be very far from the truth, but as the criterion is too severe, it seemed worth while to give the figure. The smaller mass is found to be deeply furrowed in a plane parallel to the axis of rotation, so as to be shaped like a dumb-bell, and although this result can only be taken to represent the truth very roughly, yet it cannot be entirely explained by the imperfection of the analytical method employed. It appears then as if the smaller body were on the point of separating into two masses, in the same sort of way that the Jacobian ellipsoid may be traced through the dumb-bell shape until it becomes two masses.

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* Thomson and Tait, 'Natural Philosophy,' (1883), §778 (i). He also remarks elsewhere that by thinning a Jacobian ellipsoid in the middle, we shall get a figure of the same moment of momentum and less kinetic energy.

† 'Acta Math.,' 7, 3 and 4, 1885.

M. Poincaré has commented in his paper on the possibility of the application of his results, so as to throw light on the genesis of a satellite according to the nebular hypothesis, and this investigation was undertaken with such an expectation. He remarks, however, that the conditions for the separation from a mass, which is strongly concentrated at its centre, are necessarily very different from those which he has treated mathematically.

However, both his investigation and the considerations adduced here seem to show that, when a portion of the central body becomes detached through increasing angular velocity, the portion should bear a far larger ratio to the remainder than is observed in our satellites, as compared with their planets; and it is hardly probable that the heterogeneity of the central body can make so great a difference in the results as would be necessary, if we are to make an application of these ideas.

It seems then at present necessary to suppose that after the birth of a satellite, if it takes place at all in this way, a series of changes occur which are still quite unknown.

VII. "The Influence of Stress and Strain on the Physical Properties of Matter. Part I. Elasticity—*continued.* The Velocity of Sound in Metals, and a Comparison of their Moduli of Torsional and Longitudinal Elasticities as determined by Statical and Kinetical Methods." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received April 29, 1887.

(Abstract.)

The principal object of the investigation was to ascertain whether the values of the moduli of torsional and longitudinal elasticities, as determined by statical methods, would be the same as when determined by kinetical methods, provided the deformations produced were very small.

The method of determining the modulus of longitudinal elasticity statically has been already described.* This method was applied with

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* 'Phil. Trans.,' 1883, Part I.

the greatest care to wires of piano-steel, copper, platinum-silver, silver, and platinum. The wires of copper, silver, and platinum were obtained from Messrs. Johnson and Matthey as chemically pure.

The same wires were also tested for the value of the modulus of longitudinal elasticity by the method of longitudinal vibrations. In this method the wires were fixed at both ends, and corrections, which are fully described in the paper, were made for the want of rigidity in the supports at the ends. The same method served for determining the velocity of sound in the metals piano-steel, iron, copper, German-silver, platinum-silver, silver, and platinum, with considerable accuracy. The following two tables give the results obtained:—

Table I.

Metal.	Condition.	Density.	Velocity of sound in metres per second.
Piano-steel	Unannealed.......	7·7475	5198
Iron.............	Annealed	7·6831	5096
Copper	Unannealed.. . .	8·8976	3958
German-silver	,, ...	8·6320	3860
Platinum-silver ..	,, ...	12·1900	2804
Silver	,, ...	10·4668	2801
Platinum........	,, ...	21·0500	2750

Table II.

Metal.	Condition.	Young's modulus in grams per sq. cm. as obtained by the kinetical method. e_k.	Ditto, as obtained by the statical method. e_s.	$\frac{e_k}{e_s}$.
Piano-steel ..	Unannealed.	2133 × 10⁶	2140 × 10⁶	0·997
Copper	,, .	1316	1323	0·995
Platinum.....	,, .	1623	1623	1·000
Platinum-silver	,, .	997	1001	0·996
Silver........	,, .	835·6	828·6	1·008
			Mean........	0·999

Four of the wires used in the experiments on longitudinal elasticity were also tested for torsional elasticity, both by the method of statical torsion, and by the method of torsional vibrations. The diameter of each of the wires was about 1 mm., and the lengths varied from 650 to 800 cm., thus even with very small torsional deformations, considerable accuracy was attainable. The results of these last experiments are given in Table III.

Table III.

Metal.	Condition.	Modulus of torsional elasticity in grams per sq. cm. obtained by the statical method r_s.	Ditto, obtained by the kinetical method. r_k	$\frac{r_k}{r_s}$.
Iron	Annealed..	751·5 × 10^6	766·5 × 10^6	1·020
Platinum....	Unannealed.	662·2	663·5	1·002
Silver	,, .	275·5	278·0	1·009
Aluminium ..	,, .	267·7	266·9	0·997

The general results of the whole investigation may be expressed as follows.—

1. The value of the modulus of longitudinal elasticity for hard-drawn metals, as determined by the statical method of loading, accords with the value obtained by the method of longitudinal vibrations, provided the deformations are sufficiently small.

2. The velocity of sound in a metal wire is independent of the load on the wire

3. The velocity of sound in a metal wire is not sensibly altered by permanent extension of the wire.

4. The value of the modulus of torsional elasticity, as determined by the statical method, accords with the value obtained by the method of torsional vibrations for most metals in the hard-drawn condition, provided the deformations produced are small. For annealed iron the value of the modulus obtained by the second method slightly exceeds that obtained by the first method, and by an amount which is greater than can be attributed either to the heating and cooling effects of contraction and expansion, or to errors of observation.

VIII. "On Kreatinins. I. On the Kreatinin of Urine, as distinguished from that obtained from Flesh-kreatin. II. On the Kreatinins derived from the Dehydration of Urinary Kreatin." By GEORGE STILLINGFLEET JOHNSON, M.R.C.S., F.C.S., F.I.C. Communicated by GEORGE JOHNSON, M.D., F.R.S. Received May 5, 1887.

(Abstract.)

PART I.

This investigation was suggested by a careful study of the reducing action of normal human urine upon picric acid in presence of potash at the boiling temperature.

The picric acid method for the quantitative estimation of sugar in urine was introduced by Dr. George Johnson in 1883. Whilst assisting him in working out the details of the process, the author's attention was drawn to the small amount of reduction exerted by *all* specimens of normal human urine. The average picric reduction observed was equal to that which would be effected by a solution of glucose containing 0·6 grain to 1 fluid ounce.

The reduction of cupric oxide in boiling alkaline solution was always somewhat greater, averaging 0·7 grain per 1 fluid ounce. Although many physiological chemists express the opinion that normal human urine contains always a little sugar, to which this reducing action is to be attributed, the author's researches have led him to an opposite conclusion.

The reducing agent of normal urine differs from glucose in producing some reduction of picric acid in presence of potassium hydrate at the ordinary temperature (*vide* Dr. R. Kirk, 'Lancet,' June 16, 1883).

The reducing agent of normal urine cannot be made to undergo the alcoholic fermentation in presence of yeast. Nevertheless, Dr. Pavy ('Med. Chir. Soc. Trans.,' vol. 63, p. 222) attributes one-fourth of the reducing action of normal urine upon cupric oxide to uric acid, and the remaining three-fourths to "the small amount of sugar naturally present in urine."

Brücke is also of opinion that normal human urine contains a small quantity of sugar.

Uric acid and kreatinin together are credited by Prof. E. Salkowski ('Centralblatt für die Medicinischen Wissenschaften,' March, 1886) with from one-sixth to one-fifth of the total reducing action of normal urine, the remainder being due to "other substances, and very probably to compounds of glycuronic acid (Glykuronsäureverbindungen)."

The author found that about three-fourths of the total reducing action of normal urine is destroyed by prolonged boiling with potassium hydrate, the remaining one-fourth being due to the survival of the uric acid, thus confirming Dr. Pavy's observation that one-fourth of the normal reducing action is due to uric acid.

On attempting to isolate the normal reducing agents by precipitants, it was found that mercuric chloride gradually effected complete precipitation of the reducing agent when added in sufficient excess to the unconcentrated urine. Complete precipitation can be effected in forty-eight hours if one-twentieth of its volume of a cold saturated solution of sodic acetate be first added to the fresh urine, then one-fourth of its volume of a cold saturated solution of mercuric chloride. The precipitate, which forms immediately, should be separated by filtration, as it contains no reducing agent except uric acid, which is probably present in it as mercuric urate; but the filtrate from the first amorphous precipitate begins to deposit the mercury salt of the reducing base in about half an hour. This deposit appears granular and crystalline, but under the microscope is found to consist of minute spherical masses. The weight of the dry spherical precipitate is always greater than that of the amorphous one weighed in the same condition. The desiccation must be conducted over sulphuric acid at the ordinary temperature, since the compound is decomposed at 100° C. in presence of water.

The filtrate from this granular (spherical) mercury salt is devoid of reducing action upon cupric oxide and picric acid. The spherical mercury salt of the reducing base (for the basic nature of the normal reducing agent is strongly indicated by the above results) is easily soluble in hydrochloric acid, but insoluble in acetic acid. Moistened with solution of potassium hydrate, the compound gradually blackens from reduction of mercury at the ordinary temperature, and much compound ammonia is evolved. Suspended in cold water and treated with hydrogen sulphide, the compound is decomposed, mercuric sulphide remaining undissolved, whilst the solution becomes acid in reaction, and exhibits reducing properties. On evaporating this acid solution a crystalline salt is obtained, which is the hydrochloride of the reducing base. As the spherical mercury salt, obtained as above from urine, appeared to be always quite homogeneous and pure, with the exception of a little colouring matter, the exact weight of the compound obtained from known volumes of urine was next ascertained.

The samples of urine examined were in all cases ascertained to be free from albumen and sugar before precipitation. The filtrates from the first or amorphous precipitate, produced by mercuric chloride and sodic acetate, were allowed to collect in a large glass vessel, in which the spherical compound gradually accumulated. After standing some days, the total precipitate was collected, washed with cold water, dried in a vacuum over H_2SO_4, and weighed.

(I) 34,640 c.c. of urine gave 198 grams of Hg salt, equivalent to 5·7 grams of Hg salt per litre of urine.

(II.) 40,625 c.c. of urine gave 293·13 grams of Hg salt, equivalent to 7·19 grams of Hg salt per litre of urine.

The mean specific gravity in Experiment I was 1 020, and the mean reduction of picric acid was equivalent to 0·67 grain glucose per 1 fluid ounce.

The mean specific gravity in Experiment II was 1 022, and the mean reduction of picric acid was equivalent to 0·86 grain of glucose per 1 fluid ounce.

In my own case I found that the mean quantity of mercury salt obtained from the urine of twenty-four hours agreed very nearly with the above results, the total urine of six days having been carefully collected and examined each twenty-four hours.

The formula of the spherical mercury salt arrived at by analysis is $4(C_4H_7N_3O.HCl.HgO)\,3HgCl_2$.

It may here be observed that mercuric chloride has been recommended by Maly ('Ann. Chem. Pharm,' vol. 159, p. 279) for preparing kreatinin from the urine of man or the horse. He does not mention the fractional precipitation by mercuric chloride, but recommends a preliminary concentration of the urine by heat, and precipitation by basic lead acetate before adding mercuric chloride. On repeating his process I found that even after the removal of the uric acid, &c., by basic lead acetate, the mercuric chloride still produced an immediate flocculent precipitate, followed by a granular one, and if the flocculent amorphous matter was not separated by filtration, the total mercury precipitate yielded a gummy mass after treatment with H_2S, which would not yield crystals until treated with alcohol, as Maly himself recommends. By my method, however, the hydrochloride of the reducing base is obtained in crystals after the first evaporation; these crystals contain chlorine in the proportion required by the formula $C_4H_7N_3O.HCl$.

The *platinum salt* of the reducing base is obtained in the form of anhydrous crystals, when an alcoholic solution of its hydrochloride is mixed with an alcoholic solution of platinic chloride. If these anhydrous crystals be dissolved in water, and the solution evaporated, or if the aqueous solution of the hydrochloride of the base be mixed with platinic chloride in aqueous solution and evaporated, fine orange-coloured prisms separate out, which have the formula

$$2(C_4H_7N_3O.HCl).PtCl_4.2H_2O.$$

Heated to 100° C., these crystals become anhydrous, yellow, and opaque.

The *free reducing base* is obtained from its hydrochloride by mixing the concentrated aqueous solution with excess of pure lead hydrate *without heat*, and filtering; the alkaline filtrate by spontaneous evaporation deposits large square plates with bevelled edges, or long efflorescent prisms if great care is taken to avoid heating the solution of the hydrochloride. The aqueous solution of the free base is alkaline in reaction, intensely bitter to the taste, gives crystalline precipitates with zinc chloride, mercuric chloride, and picric acid, but none with silver nitrate, unless the solution be very concentrated.

The reducing base, which is undoubtedly the natural kreatinin of urine, when anhydrous, has the empirical formula $C_4H_7N_3O$, but the efflorescent kreatinin, obtained only when great care is taken to avoid heat, has the composition $C_4H_7N_3O.2H_2O$. After efflorescence this body has the same percentage composition as the anhydrous tabular kreatinin.—

1 part by weight of the tabular kreatinin dissolves in 10·78 parts of water at 17° C.

1 part of the tabular kreatinin dissolves in 362 parts of absolute alcohol at 17° C.

The efflorescent kreatinin (before efflorescence) dissolves in 10·6 parts of water at 14° C. The effloresced kreatinin requires 14 parts of water at 14° C. for solution.

The natural kreatinin of urine reduces cupric oxide in proportion as 12 · 10 parts by weight of glucose.

The average weight of this base passed by a healthy man in twenty-four hours (as determined by weighing the spherical mercury salt precipitated from the urine as above) is from 1·7 to 2·1 grams, equivalent in reducing action upon cupric oxide to from 1·5 to 1·75 grams of glucose (= 23 to 27 grains of glucose in 52·8 fluid ounces of urine).

Therefore cupric oxide will be reduced by the normal urine in quantities equivalent to the reduction effected by 0·43 to 0·51 grain of glucose per 1 fluid ounce. The conclusion is that the total reduction effected by normal urine is accounted for by the uric acid and kreatinin which it contains.

PART II.

The natural urinary kreatinin yields a kreatin when its dilute aqueous solution is subjected to prolonged ebullition; and this kreatin, when treated by Liebig's process, is converted into kreatinin hydrochloride.

This artificial kreatinin hydrochloride differs from the hydrochloride of the natural kreatinin of urine, in that it crystallises from cold aqueous solution in efflorescent crystals, whereas the hydrochloride of the natural base is always anhydrous.

Tabular Synopsis of Comparison between various Kreatinins

	1 Efflorescent kreatinin of urine $C_4H_7N_3O\,2H_2O$	2 Tabular kreatinin of urine $C_4H_7N_3O$	3 Efflorescent kreatinin from urinary kreatin $C_4H_7N_3O\,2H_2O$	4 Tabular kreatinin a from urinary kreatin $C_4H_7N_3O$	5 Kreatinin (Liebig)
Solubility of base in water	1 in 10·6 parts at 14° C	1 in 10·78 parts at 17° C	—	1 in 10·68 parts at 16·0° C	1 in 11·5 parts at 16° C
Solubility of base in alcohol	—	1 in 362 parts at 17° C	—	1 in 324 parts at 18·0° C	1 in 102 parts at 16° C
Platinum salt	Indefinite or decomposed by alcohol	$2(C_4H_7N_3O\,HCl)$ $PtCl_4\,2H_2O$	Indefinite or decomposed by alcohol	$2(C_4H_7N_3O\,HCl)$ $PtCl_4\,2H_2O$	$2(C_4H_7N_3O\,HCl)$ $PtCl_4$
Solubility of platinum salt in water	—	1 in 14·1 parts at 1[illegible] C	—	1 in 24·4 parts at 15° C	—
Gold salt	$C_4H_7N_3O\,HCl.AuCl_3$ Unchanged by ether	$C_4H_7N_3O\,HCl\,AuCl_3$ Unchanged by ether	$C_4H_7N_3O\,HCl\,AuCl_3$ Decomposed by ether	$C_4H_7N_3O\,HCl\,AuCl_3$ Decomposed by ether	—
Reduction of cupric oxide by base compared with that of glucose	4 molecules $C_4H_7N_3O$ = 2 molecules glucose (after efflorescence)	4 molecules = 2 molecules glucose	5 molecules $C_4H_7N_3O$ = 2 molecules of glucose (after efflorescence)	o molecules 2 molecules glucose	—

Efflorescent and tabular kreatinins may be obtained from this hydrochloride, resembling in crystalline form and percentage composition those derived from the natural hydrochloride, but exhibiting some important differences. Thus the platinum salt of the artificial tabular kreatinin requires nearly twice as much water to dissolve it as that of the natural base at the same temperature.

Both the natural and artificial kreatinins form well-crystallised gold salts. The gold salt of the natural kreatinin is unchanged by ether, but that of the artificial base is decomposed thereby, the kreatinin hydrochloride separating out, and the auric chloride passing into solution.

Finally the artificial kreatinins are less powerful reducing agents than the natural base.

4 mols. of the natural kreatinins are equivalent to 2 mols. of glucose.

5 mols. of the artificial kreatinins are equivalent to 2 mols. of glucose in reducing action.

Therefore artificial kreatinins must not be compared with the natural base as to their reducing action.

Note added 13th May, 1887.

Professor W. N. Hartley has observed a marked difference in the absorption spectra of the natural kreatinin of urine, and of an artificial kreatinin obtained by the author from flesh kreatin.

IX. "On *Gasterolichenes*, a New Type of the Group *Lichenes*." By G. Massee. Communicated by W. T. Thiselton Dyer, F.R.S. Received May 5, 1887.

(Abstract.)

In the greater number of *Lichenes*, the fungal constituent belongs to the section *Ascomycetes*, characterised by having the spores produced in asci. Recently a second group of *Lichenes* has been described, and called *Hymenolichenes*, in which the fungus belongs to the Hymenomycetous *Basidiomycetes*, and closely related to the genera *Corticium* and *Stereum*. In the present communication, a third type of lichen structure is described, in which the fungus belongs to the order *Gasteromycetes*, or puff-balls. *Gasterolichenes* is the name suggested for this group, which consists of two genera, *Emericella*, Berk., and *Trichocoma*, Jungh., hitherto described as fungi.

X. "Experiments on the Discharge of Electricity through Gases. (Second Paper.)" By ARTHUR SCHUSTER, F.R.S., Professor of Applied Mathematics in Owens College. Received May 18, 1887.

Three years ago I gave a sketch of a theory of the passage of electricity through gases, in which it was assumed that in the gaseous discharge an atom carries a definite molecular charge just as in electrolysis (see Bakerian Lecture, 'Roy. Soc. Proc.,' vol. 37, p. 317). I showed how this molecular charge if constant might be measured, and I have since that time worked continuously in arranging for the necessary experiments.

I have, with the help of a grant from the Royal Society, mounted a battery which gives me an electromotive force of 1800 volts, which I hope will be sufficient for my purpose. The experiments which I have in view, involve the accurate measurement of the electric potential at different points of a vessel through which a discharge is passing; but before these could be undertaken a number of intermediate questions had to be settled by experiment. As these have in this way established some definite points which I believe to be of importance, I venture to bring them before the Society.

In thinking over the phenomena presented to us in vacuum tubes, I always felt a difficulty owing to our ignorance of the conditions which hold at the surface of bodies, either suspended in or near the discharge, or even at the boundary of the vessel through which the discharge is passing. It is evident enough, that if there is a flow of electricity on the surface of a non-conductor that flow must be tangential, but it is not so clear whether we are justified in concluding from this that there can be no normal forces at such surfaces, for it is not necessary that the flow should always take place along the lines of force. Imagine, for instance, a discharge to consist of particles charging at one pole, then moving, under the action of electric forces, to the other pole; and let the vacuum be sufficiently good that few or no encounters take place while it passes from one pole to another. Then bring an electrified body near the discharge. That body may deflect the particles conveying it, but unless its electrification is sufficiently large, it will not draw up the discharge to its surface, so that the normal forces on the electrified body which has been introduced need not be neutralised by the discharge.

The question is one altogether for experiment to decide. Supposing we suspend two pieces of gold leaf, as in an electroscope, at any place in a partially exhausted vessel, and render them divergent by electrification, they should collapse as soon as the discharge begins to pass, if tangential forces only can permanently exist at their surface. This I have tested by experiment and found to be the case.

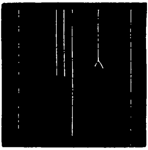
A cylindrical glass vessel, 38 cm. high and 15 cm. wide, was divided into two approximately equal compartments by a vertical metallic screen. There was an open space of about 5 mm. between the screen and the sides of the vessel, a space about 4 cm. above, and 2·5 cm. below the screen. One compartment contained two pieces of gold leaf, which could be charged from the outside. The other compartment contained two electrodes about 5 cm. apart, and 2 cm. from the screen; these distances could be varied during the experiment. The screen was always conducted to earth, and the electric fields on the two sides of the screen were therefore nearly independent of each other. When the gold leaves were electrified and divergent, and discharges from the induction coil passed between the electrodes on the other side, no effect could be observed at atmospheric pressure: the gold leaves remained divergent.

At a pressure of about 4·3 cm. of mercury, the effect I was looking for first appeared; when the discharge passed, the divergent leaves slowly collapsed, and as the pressure was further diminished the collapse took place more and more quickly.

We have here then even with the discontinuous discharge a neutralisation of all normal forces at the surface of the gold leaf.

Fig. 1 illustrates in a diagrammatic form the arrangement of the experiment, A and B being the electrodes, S the screen conducted to earth, and E the gold leaf.

FIG. 1.



In previous experiments, which were not so satisfactory from another point of view, and which I therefore do not quote, the same effects were observed both with the continuous currents from the battery and the discharges from the coil; the only difference being, that the effects due to the continuous discharge were more rapid and regular than those due to the induction coil. As soon as the pressure was sufficiently reduced to allow the continuous discharge to pass, the destruction of normal forces took place.

It seemed to me to be interesting to observe more particularly the effects of the ordinary discharges we have at our command, at atmospheric pressure. I took two light balls, and suspended them so that they could be made to diverge by electrification. The electrodes (either spheres or points) of a Voss machine were placed at a distance of 3 inches from each other, and the electrified balls were placed at a distance of 9 inches from the discharge. The results are contained in the following table, in which the two first columns indicate whether the electrodes of the Voss machine were points or spheres. The third column gives the electrification of the balls, and the fourth column the results.

Negative electrode	Positive electrode.	Balls.	Result.
Sphere	Sphere	Positive	Balls collapse slowly
"	"	Negative	" remain divergent.
Point	Point	Positive	" collapse quickly.
"	"	Negative	" remain divergent.
Sphere	"	Positive	" " "
"	"	Negative	" collapse slowly.
Point	Sphere	Positive	" " quickly.
"	"	Negative	" remain divergent.

It will be seen that when the two electrodes are similar, whether spheres or points, the balls collapse when they are electrified positively only; but that when one electrode is a sphere and another a point, the balls collapse if their electrification is of the opposite nature to that supplied by the point.

I shall refer to the theoretical bearing of these experiments at the end of the present paper, but wish at once to point out, that the apparent difference in the results, for positively and negatively electrified balls, can be one of degree only, and not one of kind. If the balls are positively electrified, they collapse when the two electrodes are similar; but in the other case, when the balls are negatively electrified, an equal and opposite charge will be found on the objects placed in the room, or on the walls. If this positive electricity is neutralised by the discharge, the balls must ultimately collapse in this case, as well as when they were originally positively electrified. To test this argument, I placed the balls in a glass case partially covered on the inside with tinfoil. The inside of the case was nearly a cubical space of sides 87 cm. long. The front of the case was taken out, and the discharge was taken between the two points near the open front. The balls now collapsed, whatever their charge was to begin with, but more quickly when they were positive than when they were negative.

Confining ourselves to the case of a discharge between points and positive balls in its neighbourhood, the question remains whether they will collapse ultimately, whatever their distance from the discharge, or whether there is a finite distance beyond which no effect can be observed, even if the discharge be continued indefinitely. Without wishing to express any final opinion on the point, I may yet give the impression I have obtained from the experiments I have made. I believe that, whenever we have a continuous and steady discharge in an inclosure, however large, the complete neutralisation of all normal forces on surfaces through which no current goes is only a question of time, but that if there is any discontinuity in the currents (as I believe is the case in all discharges at atmospheric pressure) there is a definite distance (depending on the time intervening between two discharges) beyond which no effect will be observed.

The conclusion thus arrived at, which will be proved beyond possibility of doubt in the second part of this paper, is this: *we can only have tangential forces at the surfaces of vessels enclosing a gas through which a discharge is passing*, provided no current crosses the surface. It may be that this conclusion will appear evident to some without experimental proof, but I found it necessary to obtain definite evidence, because the fact itself has been constantly neglected and disregarded.

Thus, for instance, it is found that electrified bodies placed outside a vessel through which a gaseous discharge is passing, do not permanently affect the appearance of the discharge, and this fact is commonly taken to prove that there can be no free electricity of either kind in the discharge. But it follows from the surface condition at the inside of the vessel, that this surface must act as a complete screen between the electrified bodies placed inside and those placed outside the vessel; and the experiment therefore proves nothing.

In similar fashion, Goldstein observed that certain actions of one negative electrode on another were destroyed when a screen was interposed between them. The results obtained in this paper give the obvious explanation of this fact.

After I had convinced myself that an electrified body placed in a partial vacuum through which an electric current is going, has its electricity quickly neutralised, it was doubtful still whether this neutralisation was due to an actual discharge or merely to a covering of electrified particles of an opposite sign. The question is a vital one in all cases where potentials have to be measured. For we can only measure potentials of a gas by measuring the potential of a metal in contact with it; and if an electrified body is covered by electrified particles of a different sign, there is a finite difference of potential between the metal and the gas, and we should have to inquire carefully, in each particular case, how far such a difference would affect our conclusions.

Hittorf has measured with great success the fall of potential at different points of a vacuum tube through which a discharge was passing, and none of his principal conclusions are affected by these scruples, for he gives in his paper sufficient evidence that his method is applicable to the cases he has examined. But the purposes I have in view rendered a measurement of potential necessary under severe conditions, in which a serious error might have been introduced by assuming without verification that the potential of a metal is the same as that of a gas in contact.

Strictly speaking, the potential is always continuous as long as we are dealing with finite charges, but when a layer the thickness of which extends to molecular distances has its sides charged with opposite electricity, it is customary to compare the two sides of such a layer directly with each other, and neglecting the rapid variation of potential within the layer, to speak of a discontinuity of potential. It is in this sense that I am here speaking of a possible finite difference of potential between a metal and the gas in contact.

The question is settled by the principal result of this paper:

A steady current of electricity can be obtained in air from electrodes at the ordinary temperature which are at a difference of potential of one quarter of a volt only (and probably less); provided that an independent current is maintained in the same closed vessel.

In other words, a continuous discharge throws the whole vessel into such a state that it will conduct for electromotive forces which I believe to be indefinitely small, but which the sensitiveness of the galvanometer I used has prevented me from tracing with certainty below a quarter of a volt. There cannot be therefore a finite difference of potential between a gas and a metal in contact greater than that amount.

Hittorf,* who has done more to clear up this subject than anyone else, has found already that a current from a few cells will pass crossways through a discharge in vacuo, but his auxiliary electrodes were introduced into the discharge itself, and it was doubtful, therefore, how far the results were due to the high temperature of the particles carrying the luminous discharge.† I was aiming, on the contrary, at placing the secondary electrodes as far from the main discharge as possible, and at rendering by means of screens the two electric fields as independent of each other as possible.

I need not describe all the successive experiments in which I have endeavoured to make my tests more and more severe. It will be sufficient to give an account of those experiments which I consider most conclusive.

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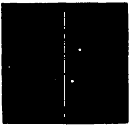
* 'Wiedemann, Annalen,' vol. 7, p. 614.

† That high temperature itself is a matter on which authorities differ. I do not desire at present to commit myself to any view which seems to me to involve a definition of "temperatures" under exceptional circumstances.

The same vessel was used as in the previous experiment; the screen was not so long, but left a space of 10 cm. free at the bottom. Fig. 2 will explain the arrangement. SS′ is the screen, always conducted to earth; A and B are the electrodes for the main discharge; C and D are the auxiliary electrodes, which were of the form of copper cylinders, 4 cm. high, and having a diameter of 1 and 1·8 cm. respectively. Their distance was 2 cm., and their axis was 2 cm. away from the screen. Of the main electrodes, one was 2 cm. and the other 1 cm. away from the screen; their distance was about 4 cm. The discharge from the battery was always used. Whenever a steady current passed between A and B, an auxiliary battery of Clark cells would send a steady current between C and D, and the lower limit of electromotive force capable of producing a measurable current seemed only to depend on the delicacy of the galvanometer.

Fig 2.



Thus, for instance, at a pressure of about ½ mm., a current of 0·11 ampère was sent from A to B; and the following currents were obtained when the poles of an auxiliary battery were connected with C and D, the galvanometer being inserted in the auxiliary circuit:—

40 Clark cells	0·032 micro-ampère.*
20 ,, ...	0·021 ,,
10 ,, ...	0·014 ,,
1 Leclanché	0·005 ,,

On another day I tried to reduce the electromotive forces still further. The galvanometer, however, had for this purpose to be rendered so astatic that the always changing torsion of the fibre suspending the mirror was a source of serious trouble; for this reason the numbers have not much value in themselves, but there was no doubt in each case as to the existence of a current. The whole effect was smaller on that day for reasons which could be traced.

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* 1 micro-ampère = 1 ampère × 10^{-6}.

1 Leclanché	gave a current of	0·0010 micro-ampère.
5/18	,, ,,	0·0011 ,,
1/6	,, ,,	0 0003 ,,

The main current in these last experiments was 0·008 ampère.

An electromotive force of one-sixth Leclanché is about one-quarter of a volt, and a current has thus been obtained in a gas from an electromotive force which could not maintain a current through water.

An electromotive force of 0·1 volt gave doubtful results, but this was probably due to the experimental difficulty of detecting the current.

In some previous experiments, which, however, were not quite free from objection on other grounds, the lowest electromotive force for which the currents could be measured was 0·2 volt.

The experimental arrangement which is the best for the qualitative investigation of the effect is not the best for quantitative measurements, and I have therefore not endeavoured to follow out to any great extent the quantitative laws of these currents produced by low electromotive forces. I may give, however, some facts which I have observed. The intensity of the current depends on a great many circumstances.

1. It increases rapidly with the intensity of the main discharge, and also with a reduction of pressure, as far as I have tried it (that is about ½ mm.).

2. The intensity of the current from the auxiliary battery increases less rapidly than the electromotive force.

3. In some experiments in which one of the electrodes of the auxiliary battery was a copper wire and the other a copper cylinder, the current was nearly always considerably stronger when the larger surface was the kathode.

4. Anything that facilitates the diffusion of gas from the main current to the auxiliary electrodes will increase the strength of the current observed. In some experiments, in which the screen separating the two fields was made of wire gauze instead of tinfoil, the currents were stronger than those given above.

5. In the arrangement shown in fig. 2 the currents were stronger when the main electrode A was negative than when it was positive.

Considerable care has to be taken, especially when no screen is used or when it is not conducted to earth, in order to avoid leakage currents. However well the battery may originally have been insulated, the insulation always grows worse with time (owing to dust and moisture). If, then, any part of the auxiliary circuit itself is not properly insulated, we easily get a current through the galvanometer which is nothing but a branch current from the main discharge. Such a leakage current, even when it is weak, considerably increases the effects described in this paper.

These experiments show conclusively that there is nothing peculiar in the gaseous state of a body to prevent any electromotive force however small from producing a current. If a finite electromotive force is required under ordinary circumstances the fact cannot be accounted for, as Edlund and others have attempted to do, by a special surface resistance which has to be overcome by a finite difference of potential at the surface.

I think the facts are very well accounted for by the theory which I have proposed in my last paper. If the two atoms of a gas making up the molecule are charged by opposite electricities, but are held together in addition by molecular forces, a finite force is required to overcome the latter. But as soon as that force is overcome and the atoms themselves are set free to diffuse and constitute a current, these atoms will be able to follow any electromotive force which we may apply. If, then, we have auxiliary electrodes, these electrodes will establish their electric field which we can never screen off completely from any other part of the vessel except by closed surfaces. The atoms, with their positive and negative charges, will diffuse across to the auxiliary electrodes and give off their electricity to them. No finite difference of potential is required in the auxiliary electrodes, because even if there is work done in making an atom interchange its positive for negative electricity, that work is undone again at the other pole, where atoms of a similar kind interchange negative for positive electricity.

This I believe to be the general explanation of the phenomena described in this paper. In order to account for the peculiar difference between positive and negative electricity which appears in the experiments done at atmospheric pressure, also those mentioned under 5 (p. 377), we must make some further supposition. I have already mentioned in my last paper that, according to the theory I have proposed, we must imagine the molecules to be broken up at the negative pole, and I believe that this fact will ultimately be found to account for this apparently unsymmetrical property of the two electricities; but I should like to strengthen my case by further experiment before going into details on this point.

I should like, in conclusion, to point out an important application of these results. I have last year obtained by calculation results which seem to show that the principal cause of the diurnal variation of terrestrial magnetism is to be looked for in the upper regions of the atmosphere. Professor Balfour Stewart at various times suggested that the air currents in these regions may, owing to the lines of force of terrestrial magnetism, have electric currents circulating in them.

The difficulty against this supposition always seemed to me to lie in the fact that the electromotive forces required to start a current were larger than those which could possibly exist in the atmosphere. But as there are very likely continuous electric disturbances going on, such as we observe in auroræ and thunderstorms, the regions within which these discharges take place would act as conductors for any additional electromotive force however small, so that any regular motion, such as tidal motions, could very well produce periodic effects affecting our magnetic needles.

If these original discharges increase in importance, then, according to the results obtained in this paper, the currents due to the smaller periodic causes would increase also, and they may increase in a very rapid ratio. We know that the electric discharges in the upper regions of the atmosphere are considerably stronger at times of many sunspots, and this may account for the fact that at those times the amplitude of the daily oscillation of the magnetic needle is considerably increased.

I have had considerable assistance in these experiments from my assistant, Mr. Stanton, to whom my best thanks are due.

XI. "Contributions to our Knowledge of Antimony Pentachloride." By RICHARD ANSCHÜTZ and P. NORMAN EVANS. Communicated by Prof. A. W. WILLIAMSON, For. Sec. R.S. Received May 5, 1887.

Some months ago[*] we showed that antimony pentachloride can be distilled, undecomposed, under much diminished pressure; our next step was the attempt to determine the vapour-density under similar conditions. The fact that the boiling point of antimony pentachloride lies much lower than that of the trichloride would seem to show that the vapour-density of the pentachloride, as in the case of the trichloride, corresponds to the simpler formula. Nevertheless, on account of the fundamental importance which the establishment of the simple formula $SbCl_5$ would have for the valence of antimony, it seemed indispensably necessary to make a determination of the vapour-density. We will preface our further observations with the remark that we have not yet succeeded in determining the vapour-density of antimony pentachloride under diminished pressure; however, in the course of many unsuccessful attempts which we have made to this end, we had one point thrust on our notice, which on investigation led to some important results concerning our knowledge of the chemical nature of antimony pentachloride.

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[*] 'Chem. Soc. Journ.,' vol. 49, 1886, p. 708.

Antimony pentachloride is well known to be an extremely hygroscopic body. Consequently in the application of La Coste's modification of Victor Meyer's method for the determination of vapour-densities under diminished pressure, it was necessary to substitute liquid paraffin for water; yet, in spite of numerous careful experiments, we could not attain our object, the liquid paraffin offered too great a resistance to the air, and before all the air had been driven out the antimony pentachloride distilled into the upper and cooler parts of the apparatus. During these experiments it was proved to us that it is next to impossible to prevent the formation of traces of the white substance, which is the result of the action of water on antimony pentachloride. We were accordingly constrained to put this method aside.

Before attempting the determination of antimony pentachloride by another method, we deemed it best first to find out the nature of the error caused by the formation of the minute amount of the product of water reacting on antimony pentachloride.

After careful consideration we were struck by the contradiction between the conclusions of Daubrawa[*] and those of R. Weber,[†] concerning the behaviour of antimony pentachloride with water. According to the latter, antimony pentachloride forms with water a hydrate, which is impossible if with 1 mol. of water antimony pentachloride is changed in the cold to $SbOCl_3$ and 2 mols. of hydrochloric acid, as Daubrawa thinks he has proved.

The following results of our experiments show that Daubrawa's statements concerning antimony oxychloride, $SbOCl_3$, are entirely wrong. In connexion with these studies we have occupied ourselves with the action of oxalic acid on antimony pentachloride, and have succeeded in obtaining the remarkable product of this reaction in a pure state.

Reaction of Water on Antimony Pentachloride.

Daubrawa allowed 1 part by weight of distilled water to drop from a pipette into a flask, surrounded by ice, containing 16 parts by weight of antimony pentachloride. He found the decomposition accompanied by hissing, and by the formation of vapour, giving no white pulverulent precipitate, but forming a yellowish distinctly crystalline mass, which adhered to the flask. This crystalline mass, the behaviour of which with water Daubrawa describes in detail, is, according to his analysis, antimony oxychloride, $SbOCl_3$, and he expresses its formation from antimony pentachloride and water by the following equation:—

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[*] 'Liebig's Annalen,' vol. 186, 1887, p. 118.

[†] 'Poggendorff, Annalen,' vol. 125, 1865, p. 86.

$$\mathrm{Sb}\left\{\begin{matrix}\mathrm{Cl}\\ \mathrm{Cl}\\ \mathrm{Cl}\\ \mathrm{Cl}\\ \mathrm{Cl}\end{matrix}\right. + \mathrm{H_2O} = \mathrm{SbOCl_3} + 2\mathrm{HCl}.$$

We proceeded exactly as above described, and joined to our flask a receiver containing a weighed quantity of water in which to collect the liberated hydrochloric acid. To 21·5 grams of well-cooled antimony pentachloride, which had been purified by distillation under diminished pressure, we added drop by drop 1·2 gram, the calculated amount, of water, and found to our surprise that no hydrochloric acid was liberated; also that the yellowish crystalline product was equal in weight to the sum of the weights of the water and antimony pentachloride originally taken

The resulting product was only partly soluble in chloroform, and was therefore a mixture of different substances.

We therefore altered the conditions of the experiment. In order to moderate the reaction we dissolved 20·4 grams of pure antimony pentachloride in about the same volume of chloroform, and added drop by drop to the cooled solution the calculated amount, 1·1 gram, of water, shaking constantly. The formation of the new body was soon apparent by the separation of almost colourless crystals; the yellow colour of the chloroform, caused by the antimony pentachloride (which according to our experience is never colourless, but always a bright yellow liquid), gradually disappeared, and at the end of the reaction the crystals which had separated were covered by colourless liquid. Under these changed conditions there was again no trace of hydrochloric acid set free. We next heated our product nearly to the boiling point of chloroform, adding sufficient dry chloroform to dissolve the crystals which had separated. From this solution a compound was deposited in feathery crystals. After decanting the mother-liquor, these were washed with a small quantity of chloroform, and placed to dry in a vacuum desiccator on a porous plate that had been previously heated. This body is so extraordinarily hygroscopic that the analyses were not very easy; however, the results leave no doubt that $SbCl_5H_2O$ is the formula:—

1. 0·2719 gram	substance	gave	0·6074 AgCl.	
2. 0·1457	"	"	0·3270 AgCl.	
3. 0·1467	"	"	0·3290 AgCl.	
4. 0·3003	"	"	0·1461 Sb_2O_4.	
5. 0·3104	"	"	0·1492 Sb_2O_4.	
6. 0·2020	"	"	0·1061 Sb_2S_3.	
7. 1·1387	"	"	0·0961 H_2O	Combustion with lead chromate.
8. 1·3905	"	"	0·1180 H_2O	

	Calculated for $SbCl_5H_2O$.	Found.							
		1.	2	3.	4.	5.	6.	7	8.
Cl...	55·90	55·26	55·52	55 48	—	—	—	—	—
Sb..	38 42	—	—	—	38·53	38·08	37 69	—	—
H...	0·63	—	—	—	—	—	—	0·93	0·94
O...	5·04								
	99 99								

We provisionally designate this body as antimony pentachloride monohydrate, without expressing any opinion concerning the constitution of this addition product of equal molecules of water and antimony pentachloride. From chloroform, the monohydrate crystallises in leafy or feathery crystals resembling sal-ammoniac, having a melting point lying between 87° and 92°. If exposed to the air it deliquesces to a clear liquid, which over sulphuric acid gradually crystallises again in broad needles, described by Daubrawa as a property of his supposed oxychloride. We have not further examined this body.

When one tries to distil the antimony pentachloride monohydrate under diminished pressure—under 20 mm. with the bath at 105°—a mobile yellow liquid distils over; this, after two rectifications, boiled constantly at 73° under 17 mm. (bath 90°) Two chlorine determinations gave, as was expected, results for antimony pentachloride.

1. 0 2510 gram substance gave 0·5965 AgCl.
2. 0·2879 „ „ 0·6885 AgCl.

	Calculated for $SbCl_5$.		Found.	
			I.	II
Cl....	59·24	....	58·80	59·15

There separated from a fraction boiling somewhat higher crystals of antimony trichloride, leaving a residue of a waxy consistency which could not be distilled.

22·7 grams $SbCl_5H_2O$ gave 12 8 grams $SbCl_5$.
„ „ 1·1 grams $SbCl_3$.
„ „ 6 4 grams residue.
„ „ 2·4 loss.

Although after this experience it was not probable that we could obtain antimony oxychloride, $SbOCl_3$, by heating antimony pentachloride monohydrate, we repeated the experiment in another form. We heated 24·4 grams of antimony pentachloride, mixed with about twice its volume of chloroform and 1·4 gram water, in a closed tube to 100° on a water-bath. The crystals of the monohydrate which were first formed went into solution, and on opening the tube before a lamp at the end of six hours we found much pressure; a gas smelling like phosgene was liberated. The tube was again closed and heated afresh to 100°. The opening and closing was repeated, first at intervals of six hours, and later, as the pressure diminished, of twelve hours, until after about fourteen days the pressure was no longer noticeable.

On working up the products of the reaction we found, besides antimony tri- and penta-chlorides, *phosgene* in solution in the chloroform, from which we prepared diphenyl carbamide, m. p. 239°. For the formation of phosgene gas it is not necessary to work in closed tubes. A chloroform solution of the monohydrate, heated to its boiling point on a water-bath, yields a steady stream of phosgene mixed with hydrochloric acid. A similar decomposition takes place when carbon tetrachloride is heated with antimony pentachloride monohydrate in a closed tube to 100°. We will communicate the quantitative relations of this reaction after we have verified our first observations by repeated research.

One can make the tetrahydrate of antimony pentachloride even more easily than the monohydrate. For this purpose we dissolved 29·3 grams of antimony pentachloride in about twice its volume of chloroform, and after surrounding the flask containing this solution with ice, let fall drop by drop 7 grams, the calculated quantity, of water. Again, in this experiment, no hydrochloric acid was given off. The mixture remained at first liquid, but when placed in a vacuum desiccator over sulphuric acid and paraffin, there separated slowly a hard crystalline mass, which was quite insoluble in chloroform. Two chlorine determinations made after washing the substance with chloroform agree with the formula $SbCl_5H_8O_4$.

1. 0·3968 gram substance gave 0·7696 AgCl.
2. 0·3414 " " 0·6530 AgCl.

	Calculated for $SbCl_5H_8O_4$.	Found.	
		I.	II.
Cl	47·77	48·04	47·31

Thus besides the monohydrate of antimony pentachloride a tetrahydrate also exists, the latter being easily prepared from a chloroform solution of the former by the action of water, and differing from the monohydrate among other things in being insoluble in chloroform.

From the foregoing evidence it appears that antimony oxychloride, $SbOCl_3$, is certainly not formed by the action of water on antimony pentachloride; but if equal molecules of these substances are allowed to react, the compound $SbCl_5H_2O$ is formed, which we have called antimony pentachloride monohydrate. This product is most conveniently formed when water acts on chloroform solution of antimony pentachloride, without setting free hydrochloric acid, according to the equation—

$$SbCl_5 + H_2O = SbCl_5H_2O.$$

When one brings together weights equivalent to 1 mol. of antimony pentachloride and 4 mols. of water, the tetrahydrate of antimony pentachloride is formed, as described by R. Weber. We must put aside Daubrawa's contradiction of the facts of R. Weber, and strike Daubrawa's hypothetical antimony oxychloride, $SbOCl_3$, from the list of the known antimony compounds.

The Reaction of Antimony Pentachloride and Oxalic Acid.

As has already[*] been remarked, our research on the chlorides of antimony sprung originally from the wish to find out if the reaction of anhydrous oxalic acid and antimony pentachloride was analogous to that of phosphorus pentachloride, forming an antimony oxychloride, $SbOCl_3$, corresponding to the oxychloride of phosphorus. The knowledge of the action of water on antimony pentachloride, forming mono- and tetra-hydrates, increased the interest in the attempt to discover the action of oxalic acid on the same body.

On mixing equal molecular weights of pure anhydrous oxalic acid and antimony pentachloride, a violent development of hydrochloric acid takes place, the mixture forming a nearly solid white mass. The generation of hydrochloric acid soon ceases. On heating to about 150° decomposition begins again, carbonic dioxide and hydrochloric acid being given off, the mass becoming gradually liquid. This clearly showed that the reaction ran in two stages. In the case of the monohydrate we had obtained our object so quickly by the use of chloroform, that we did not pursue the direct action of antimony pentachloride on oxalic acid further, but substituted a cooled solution of 35·5 grams of antimony pentachloride in 83·3 grams of chloroform, with 10·6 grams of anhydrous oxalic acid. The reaction commenced in the cold; at first hydrochloric acid mixed with a small quantity of carbon dioxide was given off, but soon only hydrochloric acid; gradually a considerable amount of a white crystalline body fell out of solution. After the development of hydrochloric acid had ceased, we heated the product of the reaction, dissolving the greater part of the crystals. We filtered hot, and beautiful transparent colourless crystals separated from the filtrate, which we had placed in a desiccator over sulphuric acid and paraffin. The residue, insoluble in the hot chloroform solution, weighed 4·9 grams, and consisted chiefly of unaltered oxalic acid. The quantity of hydrochloric acid collected was 4·7 grams, while 8·5 grams should have been found had the reaction taken place according to the equation—

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[*] 'Chem. Soc. Journ.,' vol. 49, p. 708.

$$(COOH)_2 + SbCl_5 = SbCl_3O + CO_2 + CO + 2HCl.$$

This reaction was, however, impossible, owing to the small quantity of carbon dioxide given off. The quantity of hydrochloric acid found indicated that two molecules of antimony pentachloride had reacted with one of oxalic acid. The tabular crystals from the chloroform solution melted at from 148·5° to 149°, decomposing at a somewhat higher temperature. The values found by analysis corresponded to the formula $Sb_2Cl_8C_2O_4$.

1. 0·4860 gram substance gave 0·2637 Sb_2S_3.
2. 0·2647 „ „ 0·1421 Sb_2S_3.
3. 0·1444 „ „ 0·2670 AgCl.
4. 0·2261 „ „ 0·4174 AgCl.
5. 0·3117 „ „ 0·1960 $C_2O_4Ca + H_2O$.
6. 0·2522 „ „ 0·0589 $C_2O_4Ca + H_2O$
7. 0·2956 „ „ 0·0415 CO_2 and 0·0160 H_2O } Combustion with
8. 0·8777 „ „ 0·1162 CO_2 and 0·0162 H_2O } lead chromate.

	Calculated for $Sb_2Cl_8C_2O_4$.	Found.							
		1.	2.	3.	4.				
Sb ..	39·61	38·93	38·52	—	—	—	—	—	—
Cl...	46·10	—	—	45·74	45·64	—	—	—	—
C ...	3·89	—	—	—	—	—	—	3·83	3·61
C_2O_4	14·28	—	—	—	—	14·47	14·07	—	—
H...	0·0	—	—	—	—	—	—	0·59	0·20

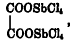
The compound, $Sb_2Cl_8C_2O_4$, is formed according to the equation—

$$(COOH)_2 + 2SbCl_5 = C_2O_4Sb_2Cl_8 + 2HCl.$$

Proceeding exactly as described above, but taking one molecule of oxalic acid to two of antimony pentachloride, *i.e.*, twice the quantity, almost the entire product is soluble in chloroform.

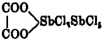
37·2 grams $SbCl_5$ dissolved in 55 grams $CHCl_3$ and
5·5 grams $(COOH)_2$ gave
4·3 grams of HCl, the calculated quantity being
4·8 grams.

It is necessary to have a good return-flow condenser in order to avoid a considerable quantity of chloroform distilling into the weighed water, used for collecting the hydrochloric acid. On mixing $Sb_2Cl_8C_2O_4$ with warm water, it is decomposed, setting oxalic acid free. This may be easily determined after removing the antimony from the acid solution. The following constitutional formula seems most simply to explain the formation and decomposition of this new compound —

I 

$$\begin{array}{l} COOSbCl_4 \\ | \\ COOSbCl_4 \end{array},$$

oxalic acid in which the hydrogen atoms are replaced by the univalent radical Sb′Cl_4 Considering the fact that the compound does not unite with a second molecule of oxalic acid, the formula—

II 

$$\begin{array}{l} COO \\ | \\ COO \end{array}\!\!>SbCl_3,SbCl_5$$

does not seem at all probable According to formula I one can compare the compound with diammonium oxalate, and designate it as ditetrachlorstibonium oxalate, or as we prefer, look on it as the mixed anhydride of oxalic acid and the yet unknown acid, $SbCl_4OH$

As a result of these simple experiments we can understand the entirely different behaviour of antimony and phosphorus pentachlorides towards the carbon compounds containing oxygen and hydrogen Antimony pentachloride unites, as we have proved, with water Phosphorus pentachloride decomposes water Antimony pentachloride has no inclination to change chlorine for oxygen Phosphorus has this inclination in an extraordinary degree Phosphorus pentachloride attacks hydroxyl or ketone groups, replacing oxygen by respectively one or two atoms of chlorine, being itself converted into oxychloride Antimony pentachloride, on the other hand, reacts on the hydrocarbon residue, substituting chlorine for hydrogen, and itself becoming antimony trichloride

Phosphorus pentachloride acts similarly to antimony pentachloride, substituting chlorine for hydrogen in those organic compounds which contain no oxygen, or oxygen in such firm combination with carbon as not to be available for the formation of phosphorus oxychloride

Antimony pentachloride may react as phosphorus pentachloride on compounds similar to oxalic acid, containing no hydrogen attached to carbon which can be replaced by chlorine The compound $(COOSbCl_4)_2$ is of especial theoretical interest, as showing the process of the reaction of phosphorus pentachloride on substances containing the hydroxyl group The first stage of the reaction consists, evidently, of the formation of compounds analogous in composition to the body $(COOSbCl_4)_2$

If, however, the oxygen of a hydroxyl group is held in loose combination, the formation of phosphorus oxychloride and the substitution of chlorine take place simultaneously What, however, will happen when the substitution of oxygen is difficult, as in phenol or the aromatic oxyacids? Attempts to answer this question are now in progress in the laboratory of the Chemical Institute of the University of Bonn, and we anticipate that the result will be the preparation of bodies having the general formula $R.OPCl_4$

XII. "Note on the Electrodeposition of Alloys and on the Electromotive Forces of Metals in Cyanide Solutions" By SILVANUS P THOMPSON, D Sc, B A Communicated by Professor G CAREY FOSTER F R S Received May 12, 1887

It is known that the electrodeposition of such alloys as brass, bronze, and German silver is not practicable from mixed solutions of the sulphates or chlorides of the constituent metals, but can be accomplished by using cyanide solutions or neutral solutions containing cyanide of potassium in excess, thereby apparently departing from the law of Berzelius that out of a solution of mixed metals the least electropositive metal is deposited first

To ascertain the cause of these facts the author has investigated—

(*a*) The electromotive forces of a number of metals in aqueous solutions of cyanide of potassium

(*b*) The dependence of these electromotive forces, in particular those of copper and zinc, upon the degree of concentration of the solution

(*c*) The variation of the electromotive forces of copper and zinc in a standard solution of cyanide of potassium at varying temperatures

(*d*) The electromotive forces of zinc and copper in a "brassing" solution consisting of the mixed cyanides of zinc and copper, having excess of cyanide of potassium present, and their variation at different temperatures

It is found that the effect of higher concentration of the cyanide solutions is invariably to increase the electromotive force of copper more than it increases that of zinc

In a cold dilute solution of cyanide of potassium the electromotive force of zinc against carbon is 1 158 volt, while that of copper against carbon is 0 948 volt, or zinc is 0 210 volt higher than copper In a boiling saturated solution of cyanide of potassium, the electromotive force of zinc against carbon is 0 768 volt, and that of copper against carbon is 1 300 volt, or copper is 0 532 volt higher than zinc

It is therefore possible to construct a voltaic battery containing one metal only, namely copper, and one electrolyte only, namely an aqueous solution of cyanide of potassium, kept hot at the anode and cold at the cathode of the cell

In cyanide solutions containing about the following number of grams of cyanide to the litre, the following were the electromotive forces observed with a carbon cathode —

Solution containing per litre					
99 4 grams		191 4 grams		1 18 grams	
Metals at 18° C					
Zinc	1 520	Copper	1 434	Zinc	1 18
Copper	1 425	Zinc	1 401	Brass	0 58
Brass	1 400	Brass	1 315	German silver	0 50
German silver	1 05	German silver	0 936	Lead	0 44
Gold	0 885	Gold	0 834	Copper	0 39
Silver	0 845	Silver	0 810	Silver	0 39
Lead	0 64	Lead	0 609	Gold	0 34
Iron	0 47	Iron	0 181	Steel	0 30
Steel	0 44	Steel	0 161	Iron	0 30
Platinum	0 27	Platinum	0 017	Platinum	0 14
Carbon	0	Carbon	0	Carbon	0

Several of the metals exhibit maximum electromotive force at an intermediate concentration

The following figures were obtained for zinc and copper in solutions of cyanide of varying strengths at 17° C —

Grams per litre	E M F zinc	E M F copper	Difference Z—C
2 9	1 158	0 948	+0 210
5 9	1 167	0 967	+0 200
11 2	1 184	1 018	+0 166
23 8	1 221	1 058	+0 163
47 7	1 269	1 130	+0 139
95 5	1 303	1 220	+0 080
191 1	1 355	1 360	−0 005

In a mixed solution of cyanides of zinc and copper there is a neutral condition where the electromotive forces of zinc and copper are equal, and this neutral condition varies with the relative amounts of metal present, with the concentration of the solution, and with the temperature The neutral temperature for a solution of given concentration is lowered by adding cyanide of potassium, and is raised by adding ammonia The neutral point, however, is not well defined, the behaviour of copper being very uncertain, in general the electromotive force of clean copper in a cyanide solution rises, in some cases as much as 0 06 volt, in a few seconds after immersion, but is rapidly though temporarily lowered on agitation

Since the degree of concentration of the solution greatly affects the electromotive force of the metal, and since in the act of deposition of a metal from its solution the concentration of the liquid around the cathode is reduced, owing to slowness of diffusion, it follows that in electrodeposition the counter electromotive force at the cathode will vary with the rate at which metal is being deposited, and will, therefore, vary with the current-density employed. And since, moreover, the variations in electromotive force due to differences of concentration are greater for copper than for zinc, it follows that in the deposition of brass from a mixed solution of cyanides of a medium concentration in which zinc is slightly more electropositive than copper, there will be a certain density of current with which the metals will be deposited in nearly equal quantities, whilst for weaker current-densities the less electropositive metal will be deposited in excess, and for stronger current-densities the more electropositive metal will be deposited in excess.

Hence to variations in the concentration of the electrolyte near the cathode are due the departures, observed with all currents except weak ones, from the law that out of a solution of mixed metals the least electropositive is deposited first.

XIII. "On the true Fructification of the Carboniferous Calamites." By WILLIAM CRAWFORD WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College and the Victoria University. Received May 17, 1887.

(Abstract.)

The true systematic position of the Carboniferous Calamites has long been a debatable subject, owing to the lack of satisfactory evidence respecting the character of their fructification. Some years ago, Mr. Carruthers and the late Mr. Binney expressed their conviction that *Calamostachys Binneyana* stood in that relationship to Calamites, a conclusion which the author was unable to accept; but in 1869 he obtained a fragment of a n[illegible] Cryptogamic fruit, of which he published an account in the 'Memoirs of the Literary and Philosophical Society of Manchester.' The central axis of this Strobilus presented so many details of structure hitherto seen only in Calamites as convinced the author that it was the true fructification of these plants.

Many years elapsed before a second example of this interesting fruit was discovered, but seven or eight specimens of it recently found in a nodule from near Oldham, have come into the author's possession; these examples are in a sufficiently excellent state of preservation to enable him to illustrate almost every detail of their structure. They not only support his previous conclusions, but they supply irresistible evidence that those conclusions are correct ones. Fortunately, at least three of the Strobili have attached to them the ends of the twigs which supported them; these peduncles are indisputably Calamites of the type to which Göppert assigned the generic name of Arthropitus, which genus several of the French Palæontologists have long insisted upon classing with the Gymnospermous plants.

The fruit is beyond question that of a true spore-bearing Cryptogam; a fact which determines the Equisetiform affinities of the entire Calamitean group; since if any members of that group might possibly have been regarded as Gymnosperms, it certainly was those of the Arthropitean type. But of all such *possibilities* there is now an end.

XIV. "On Fossil Remains of *Echidna Ramsayi* (Ow.). Part II." By Sir RICHARD OWEN, K.C.B., F.R.S., &c. Received May 20, 1887.

(Abstract.)

Since the transmission of the evidence of the large extinct species of *Echidna*, the subject of the paper ('Phil. Trans.,' 1884, p. 273, Plate 14), the discoverer of the specimen, Ed. P. Ramsay, Esq., F.L.S., has prosecuted his researches in the "Wellington bone and breccia caves, New South Wales," and has added to the mutilated subject of that paper an entire humerus, a large portion of the skull, the atlas vertebra, a tibia, and fragmentary evidences of other parts of the same skeleton—adding to the knowledge of a former existence in Australia of *Echidna Ramsayi*.

The edentulous condition, proportions, and conformation of the jaws, together with other characteristic modifications of this monotrematous genus, are repeated on the same magnified scale as in the mutilated arm-bone previously described and figured.

The predatory subject of the paper on *Thylacoleo carnifex* ('Phil. Trans.,' 1887) was discovered in the same cave, and exemplifies the leonine marsupial which contributed to the extinction of the larger phytophagous and monotrematous Mammals of the Australian Continent.

XV. "Description of a Newly-excluded Young of the *Ornithorhynchus paradoxus.*" By Sir RICHARD OWEN, K.C.B., F.R.S., &c. Received May 20, 1887.

(Abstract.)

Of this interesting and long-hoped-for discovery the author was informed by his friend and correspondent, the Baron von Mueller, F.R.S., of the Botanical Gardens, Melbourne, and shortly received the specimen from the Baron: also, further details from Mr. Le Souef, of the Zoological and Acclimatisation Society's Office, Melbourne; and from the Rev. Pastor Hagenauer, Superintendent of the Missionary Station in Gipps-Land, S.E Victoria, to whose influence with the natives science is indebted for the acquisition, as I am to Baron von Mueller for the reception, of the embryo well preserved in alcohol. The specimen is nude, an inch in length, the nostrils well opened, and between them the fleshy conical support of the horny sheath, which has been shed and by which the chorion had been torn open at birth. The mouth is a transverse slit, not produced as a beak, bounded by flexible lips, and sufficiently open to receive nutriment afforded by the group of pores excluding the secretion of the mammary gland of the pouch. The fore limbs, chiefly represented by the paws and pentadactyle, with claws sufficiently developed for adhering to the part of the pouch on which the excretory pores open. The hind limbs are less developed, have the five digits feebly indicated and clawless. A short conical-pointed tail projects between them. The elongate, flattened, natatory tail of the adult is a later development. There is no trace of navel. The skin of the trunk is uniformly smooth and nude.

If this embryo should be a male, the spur of the femoral gland is a defensive organ of later growth.

The author refrains from dissection in hopes of receiving another specimen; and, after a detailed description of the external characters of the unique specimen, refers to his paper "On the Uterine Ovum of the *Ornithorhynchus*" in the volume of the 'Philosophical Transactions' for 1834, and on the "Mammary Glands" in the volume for 1832.

XVI. "On the Nephridia and 'Liver' of *Patella vulgata*." By A. B. GRIFFITHS, Ph.D., F.R.S. (Edin.), F.C.S. (Lond. and Paris), Principal and Lecturer on Chemistry and Biology, School of Science, Lincoln. Communicated by Sir RICHARD OWEN, K.C.B., F.R.S. Received May 20, 1887.

Patella vulgata (Limpet), with its conical shell adhering to the rocks of our coasts, is well known to every sea-side wanderer. This member of the Gasteropoda has been the subject of many scientific memoirs in ancient and modern times. Amongst naturalists, Aristotle was the earliest who gave an account of some of the limpet's habits, and Cuvier was the first to describe its anatomy. In this paper the author intends to describe the chemical properties of the secretions of two problematical organs of this interesting little Gasteropod.

The author has already proved the renal function of the green glands of *Astacus fluviatilis* ('Roy. Soc. Proc.', vol. 38, p. 187); also, in conjunction with Mr. Harold Follows, F.C.S., the renal function of the organs of Bojanus in Anodon ('Chemical News,' vol. 51, 1885, p. 241; 'Chem. Soc. Journ.,' vol. 48, 1885 [Abstr.], p. 921). Since the publication of those papers, Dr. C. A. MacMunn ('Journ. of Physiol.,' vol. 7 [No. 2], p. 128) has extracted uric acid from the Malpighian tubules of insects and from the nephridia of the Pulmonate Mollusca.

I. *Nephridia of* Patella vulgata.

The nephridia of *Patella vulgata* consist of two parts, left and right lobes. The left nephridium is very small in comparison to the right nephridium. The anatomy and histology of these organs have been fully described by Prof. Lankester, F.R.S. ('Ann. Mag. Nat. Hist.,' vols. 20, 1867, and 7, 1881), J. T. Cunningham ('Quart. Journ. Microsc. Sci.,' vol. 22, p. 369), and Harvey Gibson ('Edinb., Roy. Soc. Trans.,' vol. 32, pp. 617—620).

After dissecting the nephridia from the bodies of a large number of fresh limpets, the secretions of the left nephridia were examined separately from those of the right nephridia.

Both secretions were examined chemically by two separate methods as follows:—

(*a*.) The clear liquid from the nephridia was treated with a hot dilute solution of sodium hydrate.

On the addition of hydrochloric acid a slight flaky precipitate is obtained after standing for some time. These flakes when examined microscopically are seen to consist of small rhombic plates and other forms. On treating the secretion alone with alcohol, rhombic crystals are deposited which are soluble in water.

When these crystals are treated with nitric acid and then gently heated with ammonia, reddish-purple murexide [$C_8H_4(NH_4)N_5O_6$] is obtained which crystallises in prisms.

(*b*.) Another method was used for testing the secretion of the nephridia of Patella.

The secretion was boiled in distilled water, and evaporated carefully to dryness. The residue so obtained was treated with absolute alcohol and filtered. Boiling water was poured upon the residue, and to the aqueous filtrate an excess of pure acetic acid added. After standing about seven hours, crystals of uric acid ($C_5H_4N_4O_3$) were deposited, and easily recognised by the chemico-microscopical tests mentioned above.

The secretions both of the left and right nephridia yield uric acid. It has been suggested by Mr. R. J. Harvey Gibson, M.A., F R.S.E. (in his masterly memoir on the "Anatomy and Physiology of *Patella vulgata*;" 'Edinb, Roy. Soc. Trans.,' vol. 32, pp. 601—638), that the secretions of the two nephridia may be chemically distinct. The author could not extract or detect (after a most searching investigation) the presence of any other ingredient besides uric acid in either secretion.

From this investigation, the isolation of uric acid proves the renal function of the nephridia of *Patella vulgata*.

II. *The "Liver" of* Patella vulgata.

In a paper on the Cephalopod "liver" ('Edinb., Roy. Soc. Proc.,' vol. 13, pp. 120—122), the author proved from a chemical and microscopical study of its secretion that it possesses the function of a true pancreas or digestive organ.

Since the publication of the above paper, the author has investigated the nature of the secretions of various doubtful organs of the Invertebrata.*

The "liver" of Patella is a yellowish saccular gland, and the greater bulk of this organ is encircled by the superficial coil of the intestine.

(*a*.) The secretion of the gland acts upon starch-paste, converting the starch into glucose-sugar, as proved by the use of Fehling's solution.

(*b*.) The secretion produces an emulsion with oils and fats, yielding subsequently fatty acids and glycerol.

(*c*.) When a few drops of the secretion of the gland are examined with chemical reagents under the microscope, the following reactions were observed:—On running in between the slide and cover-slip a solution of iodine in potassium iodide, a brown deposit was obtained. On running in concentrated nitric acid on another slide containing a drop or two of the secretion, a yellow coloration was formed, due to the formation of xanthoproteic acid. These two reactions show the presence of albumin in the secretion of the organ in question.

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* See Dr. Griffiths' paper, "Researches on the Problematical Organs of the Invertebrata, especially those of the Cephalopoda, Gasteropoda, Lamellibranchiata, Crustacea, Insecta, and Oligochæta."—Read before the Royal Society of Edinburgh, May 16, 1887.

(*d.*) The soluble ferment secreted by the columnar cells of the epithelium of the gland was extracted according to the Wittich-Kistiakowsky method ('Pflüger, Archiv Physiol.,' vol. 9, pp. 438—459). The isolated ferment converts fibrin into leucin and tyrosin.

(*e.*) No glycocholic and taurocholic acids could be detected by the Pettenkofer and other tests. No glycogen was found in the organ or its secretion.

(*f.*) The secretion contains leucin and tyrosin.

From these investigations the conclusion to be drawn is that the so-called "liver" of *Patella vulgata* is similar in function to the pancreas of the vertebrate division of animal life

XVII. "The Air of Sewers." By Professor CARNELLEY, D.Sc., and J. S. HALDANE, M.A., M.B., University College, Dundee. Communicated by Sir H. ROSCOE, F.R.S. Received May 21, 1887.

(Abstract).

Owing to the complaints which had been made of bad smells in the House of Commons a Select Committee was appointed in the spring of 1886 to inquire into the ventilation of the House. By that Committee the authors were instructed to make a series of analyses of the air in the sewers under the Houses of Parliament, and to report thereon. Since then they have examined a considerable number of sewers in Dundee, and have also made a number of laboratory experiments. The object of the research was to obtain a general idea of the amount of some of the more important impurities in sewer air, and to throw some light on their sources, and the conditions affecting their dissemination.

After giving a brief *résumé* of the results of the analyses which had been previously made of sewer air, the authors describe the methods they have employed, and the nature and condition of the sewers they have themselves examined.

As a result of their investigation they found—(1.) That the air of

the sewers examined was in a much better condition than might have been expected (2) That the carbonic acid was about twice, and the organic matter rather over three times as great as in outside air at the same time, whereas the number of micro organisms was less (3) That in reference to the *quantity* of the three constituents named the air of the sewers was in a very much better condition than that of naturally ventilated schools and that with the notable exception of organic matter it had likewise the advantage of mechanically ventilated schools (cf paper by the authors and Dr Anderson in 'Phil Trans, 1887) (4) That the sewer air contained a much smaller number of micro organisms than the air of any class of house, and that the carbonic acid was rather greater than in the air of houses of four rooms and upwards, but less than in two and one-roomed houses As regards organic matter however the sewer air was only slightly better than the air of one roomed houses, and much worse than that of other classes of houses (The data for all the classes of houses refer to sleeping rooms when occupied during the night)

The amount of carbonic acid found by the authors was much less than that noted by earlier observers, showing that the sewers they examined were much better ventilated than those previously investigated

On taking the average of a comparatively large number of analyses it was found that the quantity of organic matter in sewer air increased with the carbonic acid, whereas the micro organisms on the whole decreased with increase of the other constituents

With regard to the sources of the several impurities in sewer air the following conclusions are drawn —(1) The *carbonic acid* in excess of outside air may be partly due to diffusion from the neighbouring soil, but its chief source is probably the oxidation of the organic matter in the sewage and in the air of the sewer (2) The *organic matter* in excess of outside air is most probably wholly or for the most part gaseous, and is of course derived from the sewage itself (3) The *micro organisms* in sewer air come entirely, or nearly so, from the outside, and are not derived, or only in relatively small numbers, from the sewer itself This is proved by the following facts —*First*, the average number of micro-organisms in sewer air was less than in outside air at the same time —viz, about 9 in the former to 16 in the latter *Second*, the number increased with the efficiency of the ventilation. *Third*, the average proportion of moulds to bacteria in sewer air was almost exactly the same as in outside air at the same time, whereas one would expect the proportion to be very different were the outside air not the source from which they were derived, seeing that such a difference has been proved to exist in the air of houses, schools, &c *Fourth*, the naked eye appearance of the colonies from sewer air is similar to that of those from ordinary air. *Fifth*, the state of filthiness of a sewer seems to have no perceptible effect on the number of micro-organisms. *Sixth*, the view that the micro-organisms in sewer air chiefly come from outside, is in perfect agreement with what is known as to the distribution of bacteria in air. *Seventh*, results obtained in the laboratory with an experimental sewer prove that the micro-organisms present in air are diminished to nearly one-half in passing along a moist tube 5 feet long and $1\frac{1}{2}$ inch in diameter at a rate of nearly 1 foot per second. Although most of the micro-organisms in sewer air come from outside, yet there was distinct evidence of their occasional dissemination from the sewage itself. This is the case when splashing occurs, owing to drains entering the sewer at points high up in the roof. It is, therefore, of great importance that drains should be so arranged as to avoid splashing as much as possible.

In view of the fact that ordinary sewer air is to all appearance comparatively innocent as regards its micro-organisms, experiments were also made to see whether it contained any poisonous volatile base of the nature of a ptomaine. These experiments so far as they went had negative results.

Experiments as to the efficacy of ordinary water traps in preventing the escape of sewer gas into houses confirmed and extended the results previously obtained by Fergus.

Though the authors do not discuss the effect of the inhalation of sewer air on health, yet the results of the above investigation are clearly such as to make one much more suspicious as to supposed evidence of the bad effects of ordinary sewer air (at least when not vitiated by splashing), such as that examined by them.

XVIII. "On the Composition of Water by Volume." By ALEXANDER SCOTT, M.A., D.Sc. Communicated by Lord RAYLEIGH, D.C.L., Sec. R.S. Received May 23, 1887.

In 1805 Gay-Lussac and Humboldt published their classical researches on the composition of the atmosphere, and to them we are indebted for our knowledge of the proportion by volume in which hydrogen and oxygen combine to form water. Without this knowledge the determination of the relative densities of the two gases would be of no use in fixing or checking their atomic weights. This is often overlooked, and Avogadro's law taken as absolutely true for these gases at ordinary temperatures and pressures. That this cannot safely be assumed is conclusively proved by the researches of Regnault, Amagat, and others on the effects of change of temperature and pressure upon them. Not only do they not follow Boyle's law as usually understood, but their deviations from it are in opposite directions: hence it can only be by the merest chance that at our ordinary temperatures and pressures the combining volumes should be exactly two of hydrogen to one of oxygen. Moreover, when we consider that it is more than eighty years since these researches were carried out, that the instrument used in all the measurements was Volta's eudiometer, and that the gases were collected and measured over water and so contained impurities to the extent of 0·4 per cent. in the oxygen and 0·6 to 0·8 per cent. in the hydrogen used, a redetermination of this ratio with the greatly improved means for attaining accuracy now at our command seemed to be of extreme importance. The exact ratio as given in the memoir referred to is 199·89 volumes of hydrogen to 100 volumes of oxygen, and this the authors say is almost exactly as 2 : 1.

To arrive at greater accuracy the author of this note has given especial attention to the following points:—

(1.) The preparation of purer gases.

(2.) The use of larger volumes.

(3.) The measurement of both gases in the same vessel.

(4.) The analysis of the residue after explosion and determination of the impurity in each experiment.

These ends were more or less satisfactorily attained by the use of the apparatus employed, which was entirely of glass with the exception of the junctions at H and O. The gas generators contained only small volumes of gas, and could easily be exhausted by means of the apparatus itself. It is evident that by filling A and B with mercury, completely closing all the stopcocks, and then lowering M and opening *e*, the air in the oxygen generator would be in great part drawn into A; on now closing *e* and raising M this air could be expelled by opening *f*. By repeating this several times an almost perfect vacuum could be produced. Before collecting the gas for the experiments, after exhausting the air gas was evolved and exhausted, and this again repeated. The gases were measured saturated with moisture, and after measurement were expelled into G; from this they were drawn into E and exploded, and the residue measured in a small tube and analysed by explosion with either hydrogen or oxygen as required. The results of every experiment made are given in the following table, from which it will be seen that in no case, even when the maximum value is given to it, does the ratio exceed 2 vols. of hydrogen to 1 vol. of oxygen, although in four cases it is exactly 2 : 1.

The mean of the twenty-one experiments gives the ratio—

1·9857 : 1 from Column E.
1·9941 : 1 „ F.

Excluding experiments IV and VI, in which the gases contained much impurity, we get the ratio—

1·9897 : 1 from Column E.
1·9959 : 1 „ F.

Taking experiments I, III, XV, and XVIII, in which the purest gases were used, we get—

1·9938 : 1 from Column E.
1·9964 : 1 „ F.

or taking experiments I, III, XIV, XV, XVIII, and XX we get—

1·9938 : 1 from Column E.
1·9967 : 1 „ F.

Taking as the most probable ratio 1·994 : 1, and the density of oxygen referred to hydrogen as 15·9627, we get the atomic weight of oxygen as 16·01.

The oxygen was in each of the first twenty experiments prepared from potassium chlorate, and in the twenty-first from mercuric oxide

Experiment.	A		B				C		D		E	F	G
	Measured volumes of		Residue after explosion				Combining volumes, impurity being calculated as being in both in same proportion		Combining volumes, all impurity being supposed to be in the oxygen		Calculated from column C, 1 volume of oxygen combines with the following vols of hydrogen	Calculated from column D, 1 volume of oxygen combines with the following vols of hydrogen	Impurity in gas used (total)
	Hydrogen	Oxygen	Total	Oxygen	Hydrogen	Impurity $N + CO_2$	Hydrogen	Oxygen	Hydrogen	Oxygen.			
I	3757 1	1919 5	39 5	36 8	—	2 7	3755 3	1881 8	3757 1	1880 0	1 9956	1 9984	1/[illegible]
II	3848 1	2028 7	104 4	94 5	—	9 9	3841 5	1930 9	3848 1	1924 3	1 9894	2 0000	1/[illegible]
III	3902 0	2085 7	131 3	128 7	—	2 6	3900 3	1956 1	3902 0	1954 4	1 9934	1 9965	1/[illegible]
IV	3672 6	1921 9	63 4	35 0	—	28 4	3653 7	1877 4	3672 6	1858 5	1 9460	1 9760	1/[illegible]
V	3841 5	1915 7	44 1	—	32 7	11 4	3801 2	1911 9	3808 8	1904 3	1 9882	2 0000	1/[illegible]
VI	3667 9	1868 8	56 9	—	28 1	28 8	3620 6	1857 2	3639 8	1840 0	1 9494	1 9781	1/[illegible]
VII	3685 9	1876 5	22 2	12 8	—	9 4	3679 6	1860 6	3685 9	1854 3	1 9776	1 9877	1/[illegible]
VIII	3649 1	1891 5	61 1	52 8	—	8 3	3643 6	1835 9	3649 1	1830 4	1 9846	1 9936	1/[illegible]
IX	3942 6	1997 0	18 8	12 5	—	6 3	3938 4	1982 4	3942 6	1978 2	1 9867	1 9930	1/[illegible]
X	3861 3	1967 3	30 5	24 5	—	6 0	3857 3	1940 8	3861 3	1936 8	1 9875	1 9936	1/[illegible]
XI	3799 7	1930 2	29 1	23 4	—	5 7	3795 9	1904 9	3799 7	1901 1	1 9927	1 9986	1/[illegible]
XII	3857 1	1948 2	19 4	15 2	—	4 2	3854 3	1931 6	3857 1	1928 8	1 9954	2 0000	1/[illegible]
XIII	3845 4	1954 9	25 0	20 5	—	4 5	3842 4	1932 9	3845 4	1929 9	1 9880	1 9926	1/[illegible]
XIV	3780 4	1907 4	12 2	9 0	—	3 2	3778 3	1897 3	3780 4	1895 2	1 9910	1 9950	1/[illegible]
XV	3898 6	1963 5	10 9	8 8	—	2 1	3897 2	1954 0	3898 6	1952 6	1 9945	1 9966	1/[illegible]
XVI	3910 2	2003 2	44 9	40 4	—	4 5	3907 2	1961 3	3910 2	1958 3	1 9920	1 9969	1/[illegible]
XVII	3965 5	2011 5	25 6	21 6	—	4 0	3962 9	1988 6	3965 5	1985 9	1 9928	1 9968	1/[illegible]
XVIII	4012 6	1975 9	79 3	—	77 2	2 1	3934 0	1975 2	3935 4	1973 8	1 9917	1 9940	1/[illegible]
XIX	4078 7	1964 0	184 7	—	178 2	6 5	3896 1	1961 8	3900 5	1957 4	1 9860	1 9926	1/[illegible]
XX	4095 3	2006 6	87 8	—	84 5	3 3	4006 6	2007 5	4010 8	2005 3	1 9968	2 0000	1/[illegible]
XXI	3904 8	1964 5	27 8	—	12 5	15 3	3882 1	1959 4	3892 3	1949 2	1 9813	1 9968	1/[illegible]

The unit volume taken is that of a gram of mercury at 16° C The gaseous volumes are reduced to 0° C and 760 mm. pressure The residue given in B was in all cases analysed by explosion with H or O Column C gives the combining volumes if we assume the hydrogen and oxygen to be equally pure Column D of course gives a maximum value to the ratio of the volume of hydrogen combining with 1 of oxygen The gaseous impurity was almost entirely nitrogen with small quantities of carbon dioxide.

prepared from the nitrate The hydrogen was in each case prepared by electrolysis The water produced was free from any acid reaction, and no trace of the oxides of nitrogen could be detected

XIX "On Muscle Plasma" By W D HALLIBURTON, M D, B Sc, Assistant Professor of Physiology, University College, London Communicated by Prof E A SCHAFER, F R S (From the Physiological Laboratory, University College, London) Received May 24 1887

The facts described by Kuhne relating to the properties of the muscle plasma of cold-blooded animals are true in great measure for that of mammals

Admixture of muscle plasma with solutions of neutral salts prevents the coagulation of the latter Dilution of such salted muscle plasma brings about coagulation, this occurs most readily at 37—40° C Saline extracts of rigid muscle differ from salted muscle plasma in being acid but resemble it very closely in the way in which myosin can be made to separate from it myosin in fact undergoes a recoagulation This is not a simple precipitation, it is first a jellying through the liquid, the clot subsequently contracts, squeezing out a colourless fluid or salted muscle serum This does not take place at 0° C, it occurs most readily at the temperature of the body, and is hastened by the addition of a ferment prepared from muscle in the same way as Schmidt's ferment is prepared from blood The ferment is not identical with fibrin ferment as it does not hasten the coagulation of salted blood plasma, nor does the fibrin ferment hasten the coagulation of muscle plasma The recoagulation of myosin is also accompanied by the formation of lactic acid

The proteids of muscle plasma are—

1 Paramyosinogen which is coagulated by heat at 47° C

2 Myosinogen [*] which is coagulated at 56° C

3 Myoglobulin, which differs chiefly from serum globulin in its coagulation temperature (63° C)

4 Albumin, which is apparently identical with serum albumin α, coagulating at 73° C

5 Myo albumose, this has the properties of deutero albumose, and is identical with, or closely connected to the myosin ferment

The first two proteids in the above list go to form the clot of myosin, paramyosinogen is, however, not essential for coagulation, the three last remain in the muscle serum

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[*] It is on the presence of this proteid that the power of fresh muscle juice to hasten the coagulation of blood plasma depends

Paramyosinogen, myosinogen, and myoglobulin are proteids of the globulin class They are all completely precipitated by saturation with magnesium sulphate, or sodium chloride, or by dialysing out the salts from their solutions They can be separated by fractional heat coagulation, or by fractional saturation with neutral salts

When muscle turns acid, as it does during *rigor mortis*, the pepsin which it contains is enabled to act, and at a suitable temperature (35—40° C) albumoses and peptones are formed by a process of self-digestion It is possible that the passing off of *rigor mortis*, which is apparently due to the reconversion of myosin into myosinogen, may be the first stage in the self digestion of muscle

XX "Dispersion Equivalents Part I" By J H GLADSTONE, Ph D , F R S Received May 24, 1887

The idea of refraction equivalents has become familiar to those who work on the borderland of optics and chemistry, and the value of that property as a means of investigating the chemical structure of compounds is becoming more and more recognised There is a similar property, perhaps equally valuable for the same object, which has attracted little attention hitherto, I allude to the equivalent of dispersion During the last twelve months, however, I have collated old measurements of the length of the spectrum, whether made by myself or by others, and have added many new determinations, and I am now in a position to submit some of the results to the Society

The history of the subject goes back to the first paper of Mr Dale and myself upon the refraction of light,[*] in which we gave as one of the conclusions "the length of the spectrum varies as the temperature increases" In our second paper[†] we came to the conclusion that "there is no simple relation holding good for different liquids between the increase of volume and the decrease of dispersion by heat," contrary to what we found to be the case with refraction We adopted $\mu_H - \mu_A$, i e , the difference between the refractive indices for the solar lines A and H as the measure of dispersion This divided by the density gave the specific dispersion When, however, Landolt adopted the plan of calculating the "refraction equivalent," we applied the same method to what we termed the dispersion equivalent, that is, "the difference between $P\frac{\mu_A - 1}{d}$ and $P\frac{\mu_H - 1}{d}$, or more simply

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[*] 'On the Influence of Temperature on the Refraction of Light" 'Phil Trans,' 1858 p 8

[†] 'On the Refraction, Dispersion and Sensitiveness of Liquids" 'Phil Trans,' 1863, p 323

$P\frac{\mu_H-\mu_A}{d}$," where d equals the density of the substance and P its atomic weight

In two communications made to the British Association,[*] we stated that the dispersion equivalent of any substance is little affected by the manner in which it is combined with other bodies, and we gave as the mean value of CH_2 0 35 in the vinic group, but higher figures in the benzene and pyridine groups, phosphorus equal to 2 9; chlorine 0 5, bromine 1 3, and iodine 2 6 In my subsequent paper in the 'Philosophical Transactions,'[†] in which the refraction equivalents of forty-six elements were worked out, I remarked, "the question of dispersion equivalents is also of interest, the data for the investigation of the matter are given in the Appendix" But there the matter rested The paramount interest of the refraction equivalents in truth caused both the Continental observers and myself to neglect the question of dispersion, and with the exception of brief references to it in papers on Refraction,[‡] nothing was published on the subject till last summer, when I applied the measurement of dispersion to the elucidation of the chemical structure of the essential oils,[§] and afterwards in a paper at Geneva[||] I ventured to give approximate values for fifteen elements

Almost simultaneously with these appeared a paper by Bruhl,[¶] in which he endeavoured to eliminate the influence of dispersion from the refraction equivalents of highly refractive bodies In this he seems to establish the fact that for such bodies at least, the theoretical formula of Lorenz, $\frac{\mu^2-1}{(\mu^2+2)d}$, gives more uniform results than the empirical formula $\frac{\mu-1}{d}$, but he draws as one of his conclusions, "the dispersion exercised by different bodies stands in no relation which is as yet clearly recognisable and measurable either with the refraction exerted by them, or with the chemical nature of the substances" In this and a following paper[**] he gives additional proof of the worthlessness of Cauchy's dispersion formula, or any of the suggested modifications of it, to eliminate the influence of dispersion

It will be seen that Bruhl's conclusion is inconsistent with the views I have recently expressed, and the determinations I had already published, but while I am free to confess that there are many difficulties in the investigation of dispersion which have not been felt in dealing with refraction, I hold that the following conclusions are fully warranted by the data —

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[*] 'Brit Assoc Rep,' 1866 (Trans Sec, pp 10 and 37)

[†] "On the Refraction Equivalents of the Elements' 'Phil Trans,' 1869, p 27.

[‡] 'Phil Mag' vol 11, 1881 p 59 'Brit Assoc Rep,' 1881 (Trans Sec, p 591) 'Chem Soc Journ,' vol 46 1884 p 258

[§] 'Chem Soc Journ' vol 50 1886, p 609

[||] 'Archives Sci Phys Nat,' vol 16 1886, p 192

[¶] 'Liebig s Annalen' vol 235, 1886 p 1

[**] 'Liebig s Annalen,' vol 236, 1886, p 233

1st That dispersion, like refraction, is primarily a question of the atomic constitution of the body, the general rule being that the dispersion equivalent of a compound is the sum of the dispersion equivalents of its constituents

2nd That the dispersion of a compound, like its refraction, is modified by profound differences of constitution such as changes of atomicity

3rd That the dispersion frequently reveals differences of constitution at present unrecognised by chemists, and not expressed by our formulæ

In this paper my object will be to point out the uniformity that does exist, leaving apparent exceptions for future consideration

Before entering upon an attempt to determine the dispersion equivalents of the different elementary substances, it may be well to consider the difficulty which occurred at the threshold of the enquiry, and another which appears to have deterred Bruhl from prosecuting his enquiries in the direction of dispersion

The original experiments of Mr Dale and myself led to the belief that the "specific dispersion, $\frac{\mu_H - \mu_A}{d}$, slightly diminishes with increase of temperature", but more accurate experiments made on the same specimens of bisulphide of carbon, benzene, brombenzene, and mint terpene, at the temperature of the observing room in the height of summer and depth of winter, have made me less confident of this conclusion The variations are certainly within the limits of experimental error The observations of Wullner both upon bisulphide of carbon and water, those of Baille and v d Willigen upon water, as well as those of Pisati and Paternò on benzene and cymene, show that there is little, if any, appreciable difference in the specific dispersion at different temperatures The general tendency of the observations on the seventy substances which have been examined more or less carefully, appears to be that the small difference of specific refraction that exists at different temperatures is a little greater in the case of H than in that of A

Bruhl gives three cases of isomeric or quasi-isomeric bodies He measures the specific dispersion by the B of Cauchy s formula divided by the density He shows that cinnamic alcohol, $C_9H_{10}O$, and cinnamic aldehyde, C_9H_8O, both of which he conceives to contain four pair of doubly linked carbon-atoms, have a widely different specific dispersion, that allyl paracresolate and anethol, $C_{12}H_{12}O$, having four pair of doubly-linked carbon-atoms, are also quite different in dispersion, and that, on the other hand, cymol and hexahydronaphthalin, both having the formula $C_{10}H_{14}$, but the first three pair, and the second two pair of doubly linked carbon atoms, have nearly the same dispersion But if we reckon out the refraction equivalents for cinnamic aldehyde and for anethol from the numbers given in the same table, it will be seen that they are inconsistent with the supposition that these bodies have the chemical structure that he attributes to them, in fact the extremely high dispersion in each case only tells the same tale as the extremely high refraction As to the two substances of the formula $C_{10}H_{14}$ it is open to question whether hexahydronaphthalin has only two pair of doubly linked carbon atoms and the refraction equivalent calculated for each of the specimens throws some doubt upon their purity Bruhl also compares methyldiphenylamine with cinnamic aldehyde, but the presence of nitrogen in the first body, and the uncertainty as to the constitution of the second, render it unsafe to draw any conclusions from the comparison That the specific dispersion of isomeric or polymeric bodies is practically the same, except where the constitution is very different (as in aniline and picoline), was shown in my paper in the 'Philosophical Magazine' six years ago, and this must be set against the doubtful cases mentioned above

The Elements

There are but few of the elements of which the dispersive energy can be directly determined, but it so happens that two or three of these are among the most dispersive of bodies

Phosphorus was determined by Mr Dale and myself in a melted condition, and also by Damien both in that and the solid state Our observation gives 3 1, those of Damien[*] work out at 2 9 and 2 8 respectively

Sulphur —An old observation of mine on this body liquefied, gave 0 90 for E—A, and recent observations from its solutions in bisulphide of carbon give 1 2 for F—A These agree in indicating about 2 6 for H—A

Selenium —According to the observations of Sirks,[†] the refractive indices for A and D are respectively 2 653 and 2 98, taking the specific gravity at 4 5, the dispersion equivalent of this element would be the extraordinary amount of 5 67 for D—A alone

Hydrogen —Ketteler's[‡] observations give a dispersion equivalent of 0 0152 for the difference between the green line of thallium and the red line of lithium

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[*] Journal de Physique,' 1881

[†] 'Poggendorff Annalen' vol 143 1871 p 429

[‡] 'Poggendorff Annalen, vol 124, 1865, p 390

Carbon.—Schrauf's* observations upon diamond give 0·058 for the dispersion equivalent of the same range.

Iodine, in the state of vapour, or dissolved in bisulphide of carbon, gives a spectrum in which the order of the colours is abnormal.

Far more important results have been obtained from organic substances, by following a method similar to that which Landolt adopted in his determination of the refraction equivalents of carbon, hydrogen, and oxygen. The materials for such an enquiry are very abundant. They consist of the observations published by Mr. Dale and myself in 1863, and my more recent determinations published and unpublished, the very valuable lists of Landolt and Bruhl, numerous observations by Kanonnikoff, Nasini, and others. The Continental observers have usually adopted the lines α, β, and γ of the hydrogen spectrum.

On comparing the refraction equivalents of organic liquids of the fatty acid series which differ from one another by CH_2 or multiples of it, my best determinations lie between 0·33 and 0·36, averaging about 0·35 for each CH_2. On treating in a similar manner fifteen series of such bodies in Bruhl's tables, some of which contain many terms, the dispersion equivalent for γ—α works out very uniformly at an average of 0·215. This answers to 0·342 for H—A. Armstrong's cymhydrene, which is a saturated substance of the formula $C_{10}H_{20}$, has a dispersion equivalent of 3·44, giving therefore 0·344 for each CH_2. Kanonnikoff's determinations of tetraterpene and naphthene, also $C_{10}H_{20}$, give similar numbers. It may therefore be assumed that the value of CH_2 in saturated organic compounds lies between 0·34 and 0·35, answering to the well known 7·6 as the refraction equivalent of the same combination. When, however, we examine unsaturated compounds in a similar manner, we find that the value rises to at least 0·40.

Hydrogen.—While the value of CH_2 may be fairly taken at 0·34, it is more difficult to say what portion of this is due to the carbon, and what to the hydrogen. I have endeavoured to determine it, by deducting *n* times CH_2 from the paraffines C_nH_{2n+2}, by comparing the monatomic, diatomic, and triatomic alcohols, and by other similar means. The results are somewhat irregular, as might indeed be expected from the smallness of the residual figure, but give a mean of 0·04 per each hydrogen.

Carbon.—If the H_2 in CH_2 be taken at 0·08, it follows that the carbon will have a dispersion equivalent of about 0·26. This answers to the refraction equivalent of 5·0.

It is well known, especially from the researches of Bruhl, that in unsaturated organic compounds, there is an increase of refraction, for the line A, of about 2 2 for each pair of doubly-linked carbon-atoms Assuming this to be due to a different value for carbon, we obtain a refraction equivalent of 5 0+1 1, i e, 6 1 In all such cases there is a great increase of dispersion, this increase, however, is not always the same In the allyl compounds, whether determined by Bruhl, Kanonnikoff, or myself, it is uniformly very close to 0 5 In the olefines it is the same In the whole of the aromatic series it is at least 0 8 Coincident therefore with the higher refraction equivalent for carbon, we have two dispersion equivalents of about 0 26+0 25, and 0 26+0 40, i e, 0 51 and 0 66

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* 'Ueber das Dispersionsäquivalent von Diamant.' Wiedemann, 'Annalen,' vol. 22, 1884, p. 424.

It must remain for future consideration, whether there may not be an intermediate refraction equivalent, corresponding to the dispersion equivalent of 0 51

On the appearance of Bruhl's papers in 1880, I ventured to suggest that there was a still higher refraction equivalent for carbon, in those cases in which it "has all four of its units of atomicity satisfied by other carbon atoms, each of which has the higher value of 6 0 or 6 1," as in naphthalene or pyrene This view has been, and is, the subject of controversy, but on turning to the dispersion equivalents of these bodies, they are found to be always enormously high, far higher than can be accounted for by the figures with which we have hitherto been dealing

Oxygen—It has been established by Bruhl, that in the case of aldehydes and ketones, oxygen has a refraction equivalent of 3 4 As these have the general formula $C_nH_{2n}O$, and the dispersion of CH_2 is known, it is very easy to determine the dispersion equivalent of the oxygen Various determinations of these bodies give a fairly uniform result, viz, 0 18 for H—A

In the case of the alcohols, the oxygen has a refraction equivalent of only 2 8 Comparing the dispersion equivalents of the alcohols of different atomicities in the published lists, the mean value for oxygen in this condition comes out at about 0 10 Nevertheless, in the organic acids and compound ethers, the value of the two oxygens together seems rarely if ever to exceed 0 24

Chlorine—Our lists also give us the means of determining the value of chlorine in organic substances of the fatty acid series As reckoned from such substances as chloroform, chloral, ethylene, and ethylidine chloride, and bichloride of chlorethylene, the dispersion equivalent of this halogen appears to be 0 50, though in the simple chlorides of the compound radicles it appears to be a little less

Bromine—The dispersion equivalent of bromine varies in a similar way to that of chlorine As deduced from bromoform and the dibromides of the olefines, it is 1 22, but in the bromide of ethyl it is lower

Iodine—The dispersion equivalent of iodine in di-iodide of methylene was found to be 3 65 and in iodoform in solution it seems to be about the same while in the ordinary iodides of the compound radicles it is much less

Nitrogen —Nitrogen appears to have a lower value in nitriles cyanides and sulphocyanides than in organic bases but the figures obtained so far for each condition of nitrogen are not accordant The lower value however probably does not exceed 0 10 The values of NO_2 in the fatty acid series as deduced from substitution products of the alcohols glycerine mannite &c are however fairly accordant giving about 0 82

Sulphur —For the determination of sulphur we have the excellent observations of Wiedemann * and Nasini † the first on sulphur substitution products of carbonic ethers the second on many organic compounds There exist also two or three observations of my own It appears that the value of sulphur in mercaptans sulphocyanides and sulphides of ethyl butyl amyl and allyl is about 1 21 answering to the refraction equivalent of 14 0

But in bisulphide of carbon where the refraction equivalent of sulphur is 16 0 the dispersion equivalent is 2 61 and this is about the value which the element appears to have in the isosulphocyanides while the element itself dissolved in bisulphide of carbon gives 1 20 for the dispersion F—A which is equivalent to fully 2 5

These results are collected together in the following table The dispersion equivalents here given must however not be taken for anything more than approximate

Substance	Atomic Weight	Refraction Equivalent A	Dispersion Equivalent H—A
Phosphorus	31	18 3	3 0
Sulphur double bond	32	16 0	2 6
single bonds		14 0	1 2
Hydrogen	1	1 3	0 04
Carbon	12	5 0	0 26
		6 1?	0 51
		6 1	0 66
Oxygen double bond	16	3 4	0 18
single bonds		2 8	0 10
Chlorine	35 5	9 9	0 50
Bromine	80	15 3	1 22
Iodine	127	24 5	3 65
Nitrogen	14	4 1	0 10
CH_2	14	7 6	0 34
NO_2	46	11 8	0 82

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* Journ Prakt Chem vol 114 1873 p 453

† Gazz Chim Ital vol 13 p 296

It will be seen by a glance at this table, that the dispersion equivalents of the elementary substances are not in proportion to their atomic weights, or, in other words, that they have different specific dispersive energies Thus the analogous elements sulphur and oxygen, are strongly contrasted in this respect, their specific refractive energies being respectively 0 081 and 0 011 Again it will be evident that the proportion between the refraction and dispersion is not the same even in the case of analogous elements Thus taking the three halogens, the ratio between the refraction for A and the dispersion for H—A for chlorine is about 100 to 5, for bromine 100 to 8, and for iodine 100 to 15

Metals in Salts.—In 1869, as already stated I suggested that the same data from which the refraction equivalents of the metals had been determined, would be available also for their dispersion equivalents I have many observations in addition to the data then published and Kanonnikoff has been over part of the same ground, measuring the α and β of the hydrogen spectrum Unfortunately, however, the errors of observation bear so considerable a proportion to the whole amount observed, at any rate in dilute solutions, that we cannot look upon single determinations of the dispersion equivalent of a salt as of much value Thus even when great care has been taken in measurement each index of refraction is liable to an error of ±0 0001, and as the error in determining A and H may be in opposite directions $\mu_H - \mu_A$ cannot be relied upon within ±0 0002 Now among solutions of salts the specific dispersion rarely amounts to 0 02 the error of observation may therefore be more than 1 per cent, and if the salt should form only 5 per cent of the solution, the error might exceed 20 per cent Such solutions, therefore, are practically valueless for this purpose Yet it would be easy to publish a table of miscellaneous salts, the dispersion equivalents of which had been deduced from several fairly accordant observations on fairly strong solutions, or which have been corroborated from some independent source It has appeared preferable, however, to confine attention at present to the series of potassium and sodium salts, which are far the most complete and the most instructive

It is evident at a glance, that the figures in the sodium columns are invariably lower than those in the potassium columns, and that the difference is fairly uniform In regard to the refraction equivalent, it is about 3 83,* and in the dispersion equivalent it is about 0 09

It follows, that if we can determine the value of potassium, that of sodium may be at once calculated and presumably the same process may be extended to all other metals that form soluble salts

But it is not so easy to determine the value of potassium In

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* Previously determined at 3 8

	Refraction Equivalent.			Dispersion Equivalent.		
	Potassium.	Sodium	Difference.	Potassium	Sodium.	Difference.
Chloride ..	18·83	15·40	3·43	1·27	1·18	0·09
Bromide...	25·25	21·80	3·45	2·17	2·08	0·09
Iodide.....	35·78	32·52	3·26	4·42	4·33	0·09
Hydrate ..	12·60	9·26	3·34	0·79	0·72	0·07
Formate...	20·01	16·60	3·41	1·07	0·96	0·11
Acetate....	27·52	24·34	3·18	1·32	1·23	0·09
Carbonate..	28·63	22·17	2(3·23)	1·40	1·34	2(0·03)
Oxalate....	37·55	..	..	2·18		
Nitrite	18·99	15·65	3·34	1·30*	1·17	0·13
Cyanide ...	17·18	..	..	0·94		

regard to the refraction equivalent, my original determination was 8·1; but Kanonnikoff gives only 7·75, which led me, three years ago, to recalculate the observations, taking Brühl's values for oxygen, and to reduce my previous estimate to 7·85. This is determined mainly from the organic salts, and the nitrate, and cyanide. I did not draw any conclusion from the haloid salts, as the chlorine, bromine, and iodine in them appear to have somewhat higher values than what they have in organic compounds.

How are we to determine the corresponding equivalent of dispersion? From the haloid salts it would seem to be about 0·8, but it seems likely that the disturbing influence, whatever it be, which increases the refraction of the haloid salts, would affect the dispersion. The formate and acetate, $KCHO_2$ and $KC_2H_3O_2$, promise more trustworthy results, as we can subtract from their dispersion equivalents the numbers already determined for carbon, hydrogen, and oxygen. This will give respectively 0·53 and 0·44 for the dispersion equivalent of K.

If we view potassium hydrate, KHO, as water in which one hydrogen atom is replaced by potassium, water being 0·265, we obtain the value of 0·565 for K.

From the nitrite, KNO_2, by subtracting 0·82 for NO_2, we obtain 0·48 for K.

In like manner from the cyanide, KCN, by deducting 0·36 for cyanogen, we get 0·58 for K.

From the carbonate, K_2CO_3, by taking the probable value of the CO_3 at 0·60, we get 0·40 for each K.

From the oxalate, $K_2C_2O_4$, by deducting 1·00 we get 0·59 for each K.

These figures, varying from 0·40 to 0·59, are too uncertain, and too wide to give a good average I doubt if such variations can be attributed wholly to experimental error, but on the other hand, it is difficult to imagine that potassium should have more than one dispersion equivalent, while in the same series of dissolved salts it has apparently one and the same refraction equivalent I am more disposed to believe, that the uncertainty lies in the value of the radicles to which the metal is joined, but this will require a more extended research

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* This is estimated from measurements of A, F, and G, and is somewhat open to doubt, as there seems to be something abnormal in the spectrum.

It is also an important enquiry —To what extent does the modification of the dispersion equivalent affect the refraction equivalent for the line A? On this question, and others of a similar nature, I hope shortly to submit a further communication I think it will be already sufficiently obvious that the specific dispersive energy of a compound body is a physical property analogous to, but distinct from, its specific refractive energy and that it is capable in like manner of throwing light upon chemical structure

XXI ' On the Rate at which Electricity leaks through Liquids which are Bad Conductors of Electricity ' By J J Thomson, M A, F R S, Fellow of Trinity College, and Cavendish Professor of Experimental Physics in the University of Cambridge, and H F Newall, M A, Assistant Demonstrator in Physics, Cambridge Received May 26 1887

The experiments here described were undertaken to test whether the rate at which electricity leaks through a liquid which conducts electricity badly, does or does not follow Ohm s law

The method used is described later on, it consists in establishing by a battery a difference of potential of about 100 volts between the plates of a condenser, in which the dielectric is the faulty insulator to be experimented on then disconnecting the battery, and measuring with an electrometer the rate at which the difference of potential dies away

Let v_1 and v_2 be the differences of potential at the beginning and end of an interval T, and let

$$\frac{v_1}{v_2} = s$$

If c be the capacity of the condenser, q the quantity of electricity which has leaked away in the time T, then

$$v_1 - v_2 = \frac{q}{c},$$

so that $$\frac{q}{v_2} = c(x-1).$$

The rate of leak $= q/\mathrm{T}$, so that

$$\frac{\text{rate of leak}}{\text{difference of potential at the end of the interval}} = \frac{c}{\mathrm{T}}(x-1) = \Sigma, \text{ say.}$$

Now if the conduction follows Ohm's law, Σ will be constant; hence Ohm's law will be obeyed if x be constant. The tables given later on show how nearly constant x is.

To test the accuracy of the law, we have—

$$\frac{\delta\Sigma}{\Sigma} = \frac{\delta x}{x-1} = \frac{\delta x}{x}\ldots\frac{x}{x-1},$$

so that a change of 1 per cent. in x will correspond to a change of $x-1/x$ per cent. in Σ, and a deviation from Ohm's law to this extent.

The liquids tried were benzene, olive oil, carbon bisulphide, and paraffin oil. We could detect no deviation from Ohm's law for the first three of these substances, though the difference of potential fell from 500 scale divisions to 20. For paraffin oil, however, the conductivity seemed slightly greater when the difference of potential was large than when it was small. The departure from Ohm's law even in this case was small.

Quincke* has found that when the E.M.F. is comparable with that which would cause a spark to pass through the liquid, Ohm's law ceases to be even approximately obeyed. Thus, for carbon bisulphide, when the E.M.F. was 29·21 C.G.S. units, the current was 6·2; when the E.M.F. was 47·74, the current was 36; showing that with large electromotive forces the current increases much more rapidly than the E.M.F.

With the small electromotive forces which we used, the current, however, is proportional to the E.M.F., showing that when the E.M.F. is comparable with that required to produce a spark through the liquid, other methods of dissipating the energy of the electric field must exist besides those which are active in conductors conveying a current according to Ohm's law.

We found that carbon bisulphide showed a phenomenon analogous to electric absorption, the only case we know where this has been observed in a liquid dielectric.

The conductivity of all the liquids on which we experimented increases as the temperature rises, so that in this respect they behave like electrolytes.

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* 'Wiedemann, Annalen,' vol. 28, p. 529.

Description of Apparatus

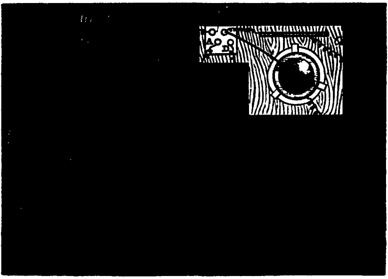
The condenser consisted of two copper cylinders, the outer one being formed into a pot 12 inches deep and 4 inches in diameter, closed at the bottom by a rounded end carefully worked inside, the inner one, 8 inches long and 3 inches in diameter, being closed, rounded off at both top and bottom, and carefully worked outside The outer cylinder was held in position on a bracket attached to a brick wall, and was always connected to earth through the gas pipes, the inner one was suspended by a silk thread, 5 feet long also from a bracket on the wall vertically above the first, and was connected to an insulated mercury cup by means of a thick wire, which was carefully soldered into the top of the cylinder, and bent into a loop to attach the silk thread to The figure shows the arrangement in elevation, the outer cylinder being represented as transparent, to show the inner cylinder and attachment

The liquid to be experimented on was poured into the outer pot, and the inner cylinder was lowered into it, both being set vertical by means of a plumb line It was found necessary to load the inner cylinder with shot to keep it sunk in the liquid

The suspension by a single silk thread was found very satisfactory, as it insulated well, and avoided the introduction of solid dielectrics, and the suspended cylinder if disturbed came again to rest after a very few oscillations

The condenser was charged by means of a number (varying between 20—80) of Post Office Daniell cells, and Thomson s quadrant electrometer (White's form) was used to show the fall of potential as the charge leaked through the faulty insulating liquids.

To facilitate changes of connexion between the condenser, the electrometer, and the cells, a paraffin block, with holes bored through it and filled with clean mercury, was used. The whole arrangement of apparatus was as shown in fig. 2.



In testing the arrangement the condenser was used as an air-condenser, and it was found that there were signs of some electrical absorption. These were traced to the paraffin key and the wire connecting it with the electrometer. The key was improved by the use of very clean mercury, and very careful amalgamation of the ends of the copper wire dipping into the mercury cups. The wire at first used to connect the cup A in the key with the electrometer was covered with gutta-percha, and the whole passed through lead pipe, put to earth to protect it electrically from the experimenter. But it was found that there was absorption on charging and the appearance of residual charge on discharging the electrometer; the gutta-percha-covered wire was therefore discarded, and bare wire stretched between insulating points, and this was then protected electrically by a shield of zinc, enclosing it and put to earth.

In testing, the condenser was connected with the electrometer by laying a piece of wire, so as to connect the cups C and A. A second piece of wire carefully insulated in a bar of paraffin which was attached to a metal handle was then used to connect the cells by the cup B to either A or C, and the condenser and electrometer were then charged, the cells were then disconnected, and the fall of potential as noted by the electrometer readings was observed at equal intervals A metronome was used to beat half seconds, the position of the spot of light on the scale was noted every 5 seconds generally, and a curve was then plotted with the time for abscissæ and the potentials as denoted by the electrometer deflection for ordinates

In preliminary experiments, in which a sample of benzene (later experiments showed it to be very impure) was used in the condenser, it was found desirable to introduce an air condenser, so as to diminish the rate of fall of potential, and to get more reliable readings The air condenser consisted of two worked brass plates, held very slightly apart by three dots of shellac One plate was attached to the inner cylinder, the other connected to earth with the outer cylinder of the experimental condenser

From time to time throughout the course of the experiments, which were continued through about three months, the apparatus was tested for leaking (i) through the electrometer, (ii) through the key, (iii) through the air condenser, (iv) through the attachments of the experimental condenser

To get rid of dirt and dust from the liquids to be placed in the condenser, very careful filtering was necessary, but generally after ten or twelve times, first through Swedish filter-paper, and finally through a tight plug of "glass wool," as much as could be done by filtering had been done

Difficulties were at first met with on account of what were probably chemical impurities in the samples used We finally decided to use much smaller quantities of the liquids in the condenser There was scarcely ever more liquid than would half immerse the inner cylinder, that is, more than would extend 4 inches up the straight part of it, but there was never less than would extend three eighths of an inch up the straight

Mode of Entering Results of Experiments

The potential of the inner cylinder was noted at equal intervals of time (generally five seconds) from the deflection of the electrometer needle These deflections were recorded in tables, and the ratios of succeeding deflections were deduced Two curves are drawn, one showing the fall of potential with the lapse of time, the other showing the value of the ratio of successive values of the potential The mean value of the ratios is found, and a line drawn with this value This cuts the curve of ratios, and shows at a glance the departures from the constant ratios, which would obtain if the leak through the dielectrics took place according to Ohm's law

Experiments on Benzene.

The first successful experiments were made with an impure sample of benzene, and the method was shown to be practicable. But before more careful readings were taken the cylinders had to be re-worked, because of some irregularities on the surfaces. On setting the apparatus up again, it was found that the impure benzene no longer insulated well enough to allow us to get readings. The reason of the change we have not been able to discover.

A pure sample of benzene was got, and after the usual filtering processes, the condenser was filled and charged, and readings were taken at five seconds intervals.

Tables I—IV give the electrometer deflections, and the ratios of successive pairs for different sets of readings under varying conditions, which are specified at the head of each table.

Tables II and IV are shown graphically in fig 3, which gives two curves of falling potential and two ratio curves.

Table I.

Experiment 12. Pure Benzene ¼ inch up straight.

Deflections	Ratios.	Deflections.	Ratios.	Deflections	Ratios.
440	1·11	212	1 09	103	1 11
396	1 10	191	1 09	93	1 11
357	1 11	174	1 12	84	1·09
321	1·11	155	1 11	77	1 10
290	1·11	140	1·11	70	1 11
261	1 11	126	1 09	62	1 08
235	1·10	115	1 11	57	1 11
				51	

Table II (see Fig. 3)

Experiment 13. Pure Benzene. ⅜ inch up straight.

Deflections	Ratios.	Deflections	Ratios.	Deflections.	Ratios.
500	1·069	220	1 070	106	1·070
468	1·073	214	1·070	99	1·087
436	1·076	200	1·081	91	1·070
405	1·077	185	1·069	85	1·075
376	1·071	173	1 074	79	1·067
351	1 073	161	1 073	74	1 088
327	1 072	150	1·095	68	1·079
305	1·077	137 ?	1 054	63	1·050
283	1 075	130 ?	1 083	60	1 071
263	1·073	120 ?	1 071	56	1 076
245	1 069	112	1·056	52	

Table III

Experiment 16 Pure benzene 2 inches up straight

Deflections	Ratios	Deflections	Ratios	Deflections	Ratios
500	1 108	195	1 128	81	1 112
412	1 133	173	1 111	72	1 100
330	1 111	155	1 122	65	1 101
350	1 123	137	1 096	59	1 113
310	1 123	125	1 136	53	1 081
274	1 123	110	1 100	49	1 113
242	1 110	100	1 129	44	1 100
218	1 122	89	1 098	40	

Table IV (see Fig 3)

Experiment 17 Pure Benzene 2 inches up straight

Deflections	Ratios	Deflections	Ratios	Deflections	Ratios
500	1 13	205	1 15	89	1 12
440	1 12	180	1 12	79	1 12
385	1 13	160	1 13	70	1 09
339	1 13	141	1 12	64	1 12
300	1 15	126	1 14	57	1 10
261	1 13	111	1 10	51	1 08
230	1 12	100	1 12	47	

Fig 3

Curves plotted for benzene from Tables II and IV

The straight lines through the ratio curves are drawn to show the mean value of the ratios

In the ratio curves 1 division of the ruled paper corresponds with 0 01 in the ratio-values

In the deflection curves 1 division = 20 in the deflection

Experiments on Paraffin Oil.

Little or no difficulty was met with in the case of the paraffin used. It was an ordinary sample of the oil used in lamps. After careful filtering it was put into the condenser, and readings were taken. These are given in Tables V—VII. Experiments 36 and 37 gave readings almost identical throughout. They are represented graphically in fig. 4. The interval between successive readings was 5 seconds.

Table V (see Fig. 4).

Experiments 36 and 37. Paraffin Oil. 1 inch up straight. Condenser charged for 10″ only before readings were taken.

Deflections.	Ratios.	Deflections.	Ratios.	Deflections.	Ratios.
420	1·183	136	1·172	55	1·170
355	1·183	116	1·160	47	1·175
300	1·170	100	1·163	40	1·143
257	1·184	86	1·170	35	1·13
217	1·162	73	1·177	31	1·14
187	1·169	62	1·127	27	
160	1·179				

Table VI.

Experiment 38. Paraffin Oil. 1 inch up straight. Condenser charged for 3′ 0″ before readings were taken.

Deflections.	Ratios.	Deflections.	Ratios.	Deflections.	Ratios.
420	1·166	170	1·156	75	1·15
360	1·161	147	1·140	65	1·16
310	1·169	129	1·163	56	1·12
265	1·150	111	1·156	50	1·13
230	1·159	96	1·129	44	1·13
199	1·161	85	1·13	39	

Table VII.

Experiment 39. Paraffin Oil ? 1 inch up straight. Short Charge.

Deflections	Ratios	Deflections	Ratios	Deflections	Ratios
450	1·184	176	1·165	84	1·150
380	1·169	151	1·161	73	1·140
325	1·161	130	1·150	64	1·163
280	1·166	113	1·153	55	1·172
240	1·170	98	1·166	47	
205	1·164				

FIG. 4.

Curve plotted for paraffin from Table V. Same scales as in fig. 3.

Experiments on Carbon Disulphide.

When carbon disulphide was used as the leaking dielectric, considerable discrepancies appeared; the ratio curve fell or remained steady in what seemed a capricious manner. Filtering seemed to make no difference, although again and again repeated; the condenser cylinders were cleaned carefully; the liquid was distilled several times, and the discrepancies only seemed to be exaggerated, especially in experiments made very soon after distillation. The still was cleaned and again used, but to no effect. The irregularities still remained, the ratio curve in some instances being as steadily horizontal as in the case of benzene and paraffin oil; at other times the values of the ratios falling as much as 12 or 15 per cent. Fig. 5 gives Table VIII graphically, showing the great fall of the ratio curve. The changes in the zero of the electrometer were such as to lead us to the notion that we had to deal with something analogous to electric absorption. We therefore tried the effect of varying the time of charging the

Fig 5

Curve plotted for carbon disulphide from Table VIII
Time of charge probably short

condenser, and in this way found that the discrepancies were due to accidental differences in the time of charge

Fig 5 shows curves drawn from carbon disulphide, the only variable in the circumstances for the different curves being the time of charge It will be seen that the curve corresponding to a long charge is much less steep than that corresponding to a short charge Tables VIII—XI give the readings and ratios for the carbon disulphide experiments

Table VIII (see Fig 5)

Experiment 27 Carbon Disulphide 2 inches up straight Time of charging not noted but from form of ratio curve probably *short*

Deflections	Ratios	Deflections	Ratios	Deflections	Ratios
400	1 28	115	1 19	41	1 17
312	1 25	96	1 20	35	1 17
250	1 25	80	1 19	30	1 20
200	1 19	67	1 17	25	1 18
168	1 21	57	1 18	22	1 16
139	1 20	48	1 17	19	

Table IX.

Experiment 30. Carbon Disulphide. ½ inch up straight. Time of charging not noted, but probably *long*.

Deflections.	Ratios.	Deflections.	Ratios.	Deflections.	Ratios.
500	1·06	302	1·05	191	1·06
470	1·07	288	1·06	180	1·04
440	1·05	273	1·05	173	1·05
419	1·06	260	1·05	165	1·04
395	1·05	248	1·05	156	1·05
375	1·05	235	1·06	149	1·05
355	1·06	222	1·05	141	1·05
335	1·05	211	1·04	134	
319	1·05	202	1·06		

Table X (see Fig. 6).

Experiment 33. Carbon Disulphide. 3 inches up straight Time of charging 2 seconds.

Deflections.	Ratios.	Deflections.	Ratios.	Deflections.	Ratios.
500	1·15	265	1·11	156	1·10
430	1·13	238	1·11	142	1·12
380	1·13	213	1·11	127	1·10
335	1·12	191	1·10	115	1·09
298	1·12	173	1·11	105	

Table XI (see Fig. 6).

Experiment 34. Carbon Disulphide. 3 inches up straight. Time of charging 10 minutes.

Deflections	Ratios.	Deflections	Ratios.	Deflections.	Ratios.
500	1·081	234	1·063	115	1·074
468	1·063	220	1·059	107	1·049
440	1·060	208	1·055	102	1·051
415	1·064	197	1·068	97	1·077
390	1·054	185	1·063	90	1·058
370	1·056	174	1·054	85	1·062
350	1·057	165	1·061	80	1·066
331	1·057	155	1·061	75	1·056
312	1·058	146	1·058	71	1·059
295	1·064	138	1·061	67	1·063
277	1·056	130	1·065	63	1·066
262	1·060	122	1·060	59	
247	1·055				

FIG 6.

Curves plotted for carbon disulphide from Tables X and XI

Upper curve from readings taken after a long period (10 minutes) of charging the condensers lower curve after a short period (2 seconds) Otherwise conditions similar

The curves of ratios are on the same scale and have the same zero line about 3 inches below the lower edge of the figure

We deal later with further experiments on these phenomena of absorption and residual charge

Experiments on Olive Oil

Very little trouble was experienced with olive oil The sample used proved to be the best insulator of all the liquids we tried After filtering once through Swedish paper and once through a glass wool plug, it insulated as well as after numerous filterings

Readings of the deflections were taken as before every five seconds, but in Tables XII and XIII only those taken at intervals of 25 seconds are recorded the former being taken after a short time of charging, the latter after a long time Fig 7 is plotted from Table XIII, but is on a different scale from figs 3—6, so far as the *time* coordinate is concerned in order to bring more of the curve into the plate

Table XII (see Fig 7)

Experiment 44 Olive oil 1 inch up straight Time of charge *short*

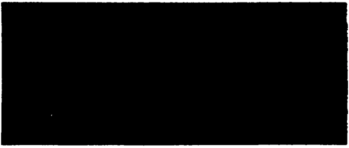
Deflections	Ratios	Deflections	Ratios	Deflections	Ratios
500		209		91	
	1 19		1 19		1 16
417		176		78	
	1 19		1 19		1 18
349		148		66	
	1 18		1 16		1 14
295		127		58	
	1 19		1 19		
247		107			
	1 18		1 17		

Table XIII.

Experiment 45. Olive oil. 1 inch up straight. Time of charge *long*.

Deflections.	Ratios.	Deflections.	Ratios	Deflections.	Ratios
500	1·196	210	1·173	107	1·202
418	1·190	179	1·185	89	1·202
351	1·185	151	1·170	74	1·200
296	1·184	129	1·205	66	
250	1·190				

Fig 7.



Curve plotted for olive oil from Table XII
Time of charging condenser short

Experiments on Rate of Leak at Different Temperatures.

In all the liquids experimented on (benzene, olive oil, carbon disulphide) the leak was quicker at higher temperatures than at lower.

These liquids then must be classed in this respect with electrolytes, and not with metallic conductors.

A metal vessel was put round the outer cylinder of the condenser, and was connected by a tube with a second vessel, which could be heated or cooled, and raised or lowered. By these means the condenser could be surrounded by hot or cold water without having its position disturbed in any way.

Readings were then taken at 15 seconds intervals, the condenser being in one set of observations under the same circumstances in every way except with respect to temperature. Each set contained three (or five) readings, one reading at a high or low temperature being taken between two readings at a medium temperature, so as to show that no permanent change had taken place by the raising or lowering the temperature of the leaking dielectric.

Benzene.—Tables XIV—XVIII give the readings for benzene at the temperatures specified at the head; and fig 8 shows the tables graphically

Benzene at different Temperatures

Table XIV		Table XV.		Table XVI.		Table XVII.	Table XVIII.	
Temp 10 8° C		Temp 22° C		Temp 11 4 C		Temp 5 4 C	Temp 8 2 C.	
480		190		190		190	190	
	1 12		1 16		1 10			1·10
438		421		444		182	444	
	1 11		1 15		1 09			1 09
395		365		405		176	407	
	1·11		1 16		1 09			1 09
356		315		369		171	374	
	1 10		1 15		1·10			1 08
324		272		335		465	345	
	1 10		1·16		1 09			1 08
295		235		306		460	318	
	1 10		1 15		1 10			1 09
267		204		278		457	292	
	1 10		1 17		1 09			1 09
243		175		254		Benzene partially frozen in the condenser	268	
	1·11		1·16		1·10			1·08
219		151		231			217	
	1 08				1 10			1 08
202				210			228	
	1 10				1·10			1 09
183				192			210	
	1 09				1 10			1 09
168				175			193	
	1 09				1 10			1 09
154				158			177	
	1 10				1 10			1 09
140				143			163	
Ratios—								
Mean..	1·10		1·157		1 097	..		1 088
Max ..	1 12		1 17		1 10	..		1·10
Min ..	1·08		1·15		1·09	..		1·08

Fig. 8

Curves plotted for benzene at different temperatures from Tables XIV, XV, XVII, and XVIII.

Dotted curve for benzene partially frozen in condenser

In the tables the values of the ratios have been put down, and they will be seen to be fairly constant in each table

Table XVII contains readings for benzene at 5 4°, the leak was so slow in this case as to attract attention, and it was found that the benzene was partially frozen hence the curve in fig 8 is dotted to show that the benzene was in a different state

The curve corresponding to Table XVI would come very close to that corresponding to Table XIV, and is therefore omitted from fig 8

Carbon Disulphide—Tables XIX—XXII give the readings and ratios for carbon disulphide at different temperatures

Carbon Disulphide at different Temperatures

Table XIX		Table XX		Table XXI		Table XXII	
Temp 9 8° C		Temp 10 2° C		Temp 4 4° C		Temp 8 6° C	
500	1 06	500	1 08	500	1 05	500	1 05
472	1 06	463	1 07	475	1 05	474	1 06
447	1 05	430	1 08	452	1 05	450	1 06
425	1 06	398	1 07	431	1 05	425	1 05
402	1 06	370	1 07	412	1 05	404	1 05
380	1 06	345	1 07	391	1 04	383	1 06
360	1 06	320	1 07	374	1 05	362	1 05
341	1 05	298	1 07	357	1 06	345	1 06
325	1 06	276	1 07	339	1 04	327	1 06
307	1 05	257	1 07	325	1 05	310	1 05
292	1 05	239	1 07	310	1 04	295	1 06
277	1 05	223	1 07	296	1 05	279	1 06
264	1 04	208		281	1 04	264	1 05
250	1 05			269	1 05	251	1 06
238	1 05			256	1 05	236	1 05
226	1 05			244	1 04	224	1 05
215	1 05			234	1 04	213	1 06
204				224	1 06	201	
				211	1 05		
				201			
Ratios—							
Mean	1 053		1 071		1 052		1 055
Max	1 06		1 08		1 06		1 06
Min	1 04		1 07		1 04		1 05

FIG. 9.

Curves plotted for carbon bisulphide at different temperatures from Tables XIX, XX, and XXI.

A curve plotted from Table XXII would almost coincide with that from Table XIX.

In fig. 9 the curve from Table XIX is practically the same as that from XXII; the latter is therefore omitted.

Olive Oil.—Tables XXIII—XXVIII give the readings and ratios for olive oil, and fig. 10 shows graphically the first three of these tables.

Olive Oil at different Temperatures.

Table XXIII.		Table XXIV.		Table XXV.	
Temp. 11·5° C.		Temp. 47·5° C.		Temp 18·5° C.	
490	1·10	? 390	2·36	490	1·24
444	1·10	165	2·54	394	1·23
403	1·10	65		319	1·53
364	1·10			260	1·24
332	1·10			210	1·24
301	1·10			168	
273	1·10				
247	1·10				
223	1·10				
203					
Ratios—					
Mean ..	1·10		2·45		1·236
Max. ..	1·10		2·54		1·24
Min. ..	1·10		2·36		1·23

Table XXVI

Temp. 10° C.	
490	
433	1·13
382	1·13
336	1·14
298	1·15
265	1·12
233	1·13
207	1·12
Ratios	
Mean ..	1·134
Max ..	1·16
Min ..	1·12

Table XXVII

Temp 31° C.	
490	
321	1·53
215	1·49
	1·51
	1·53
	1·49

Table XXVIII.

Temp. 12° C.	
490	
422	1·16
363	1·16
315	1·15
270	1·16
235	1·14
205	1·15
177	1·16
	1·156
	1·16
	1·14

Fig 10

Curves plotted for olive oil at different temperatures from Tables XXIII and XXV The dotted curve is got by a process of interpolation roughly from Table XXIV.

At the higher temperatures the rate of leak was so great that there is some doubt as to the first reading of the deflection. hence the discrepancies in the values of the ratios

Tables XXIV and XXVII are abridged from fuller tables which give readings taken at 15 seconds intervals. These are given below.

Table XXIV.

Temperature 47·5° C.	
390	1·34
290	1·32
220	1·33
165	1·32
125	1·37
90	1·38
65	

Table XXVII.

Temperature 31° C.	
490	1·15
423	1·15
369	1·15
321	1·14
282	1·15
246	1·14
215	1·14
189	

Experiments with Higher Electromotive Forces.

In the experiments dealt with so far the battery used to charge the condenser was one of 20 silver chloride cells, hence of electromotive force of about 20 volts. For electromotive forces between 20 volts and zero Ohm's law is shown to hold good for the liquids experimented on. By the following experiments the limits were extended to between about 100 volts and zero.

The condenser was charged to about 100 volts, and readings were taken at intervals of 15 seconds, the electrometer having been reset so as to be less sensitive. The ratios are, without any obvious reason, less regular than in the earlier experiments.

Table XXIX

Experiment 103. Olive oil. Charged to about 100 volts.

Deflections	Ratios	Deflections	Ratios	Deflections	Ratios.
490	1·12	201	1·16	79	1·13
437	1·11	173	1·16	70	1·15
395	1·13	150	1·15	61	1·17
349	1·14	130	1·13	52	1·13
305	1·15	115	1·14	46	1·13
265	1·13	101	1·12	40	1·14
234	1·16	90	1·14	35	1·13
				32	

Ratios—

Mean	1·14
Maximum	1·17
Minimum	1·11

Table XXX

Experiment 108 Olive oil Charged to about 100 volts

Deflections	Ratios	Deflections	Ratios	Deflections	Ratios
198	1 25	88	1 21	40	1 17
160	1 23	73	1 20	34	1 21
130	1 20	60	1 25	28	1 22
108	1 22	49	1 17	23	1 21
				19	

Ratios—
Mean 1 214
Maximum 1 25
Minimum 1 17

Experiments on Residual Charge

Carbon disulphide showed peculiarities in the above experiments With a short and sudden charge the rate of fall of potential was much quicker at the beginning than at the end of the readings This is what would be expected if there were electrical absorption part of the fall being due to true leaking part to absorption If the rate of leak is according to Ohm s law the ratio curve for such a liquid would be inclined to the horizontal at first but the inclination would diminish with time This is what is found in the case of carbon disulphide when the condenser is charged for a short time

Again the condenser with carbon bisulphide between the cylinders, was charged for a time then quickly discharged and its inner cylinder connected with the electrometer The deflection was at first connexion zero then shortly rose to a maximum value and finally diminished again after some time to zero If the condenser was charged with opposite sign the deflection from zero was in the opposite direction If the condenser was charged first with one sign and then with the other the deflections from zero were much smaller but they appeared in the sense expected from residual charge phenomena

These effects were greatest just after the CS_2 had been redistilled but at times were totally absent The still was cleaned but the effects after fresh distillation were as marked as before

Attempts to increase the effects by rendering the liquid less homogeneous were successful for heating or cooling the condenser unequally always exaggerated the deflections

The following readings are a sample of numerous experiments —

Time		
0 0′	Condenser connected to battery	
0 30	Condenser quickly discharged and connected with electrometer	
	Reading on scale	539
1 0	"	615 maximum
2 0	"	557
3 0	"	.. 544

Other liquids were tested in this way, but in no other case were similar phenomena observed. Mixtures of CS_2 and benzene or paraffin were also inactive in this sense, even when the mixtures were incomplete and the liquids were put in in such a way as to be "streaky" as was found possible.

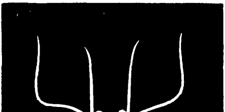
Some attempts were made to discover traces of polarisation, but no definite results were obtained. In the earliest experiments something of the kind was observed, but this was traced to the key and connexions.

XXII. "The Development of the Branchial Arterial Arches in Birds, with special Reference to the Origin of the Subclavians and Carotids." By JOHN YULE MACKAY, M.D., Senior Demonstrator of Anatomy, University of Glasgow. Communicated by Professor CLELAND, M.D., F.R.S. Received May 29, 1887.

(Abstract.)

According to the theories of Rathke, which are universally accepted at the present day, the subclavian artery is supposed to take its origin from the aortic root or fourth embryonic branchial arterial arch. In the adult bird the subclavian on each side is found springing from the extremity of an innominate artery along with the common carotid. It is presumed that the right subclavian has been, by a shortening of the aortic arch, carried forwards until it meets and fuses with the base of the common carotid artery; and the left subclavian is regarded as representing by its basal portion the fourth left arch or left primitive aorta. The subclavian of birds is thus regarded by Rathke as being developed in a manner similar to that of mammals. The author points out, however, that there is a marked difference in the relations of the artery to the surrounding parts in these two groups. In mammals the subclavian artery is crossed on its ventral aspect by the jugular vein and the pneumogastric nerve, and the recurrent branch of the latter turns round it upon the right side, but in birds the nerve

and vein are dorsally placed as regards the artery, and the recurrent laryngeal nerves turn round the ductus arteriosus or vestiges of the fifth arches a relationship which cannot be accounted for by supposing with Rathke that the vessel takes its origin first from the aortic root because if so arising it would occupy a position dorsal to vein and nerve and it is impossible to imagine a method by which the artery could pass from the dorsal to the ventral aspect of these structures without cutting them through in its course



VI Ventral vessel S¹ Subclavian of Mammals
DI Dorsal vessel S Subclavian of birds

In these circumstances the author has undertaken an investigation into the manner in which the subclavian artery first makes its appearance in birds He finds that it occupies from the first a ventral position arising from the truncus arteriosus at the ventral end of the third arch The vessel may be seen in the freshly removed embryo duck or chick on the third or fourth day, at a time when the pectoral limb is merely a small projection from the body-wall, and on the fifth day in the chick it may, while still filled with blood, be traced by the eye from the ventral end of the third arch across the superior cardinal vein to the limb The presence of this ventral vessel is also demonstrated during the third, fourth and fifth days in the chick by microscopic sections of hardened embryos, but, owing to the oblique course which the artery holds in the body-wall, it is impossible in one series of sections to trace its entire length, and this is probably the cause of its having been overlooked by previous observers In chicks at the close of the sixth day and in older forms it is pointed out that the artery may be followed by dissection under water The innominate artery has been observed to be formed, not by the gradual fusion of the subclavian and carotid arteries at their bases, but by the splitting up of the truncus arteriosus into canals continuous with the three permanent arches the innominate artery belonging to the third, and the basal portions of the aorta and pulmonary artery to the fourth and fifth respectively

The author has also examined the relations of the subclavian artery in the different groups of vertebrate animals, and finds that instead of there being but one artery prolonged into the limb, as Rathke held, there are in reality two such vessels One is represented by the mammalian subclavian, and also in lizards and amphibians is found arising from the aortic root, and passing outwards to the limb dorsal to the pneumogastric nerve and jugular vein The other, present in birds and in crocodilian and chelonian reptiles, arises from the ventral end of the third arch, and crosses outwards ventral to vein and nerve In most of the lower forms representatives of both vessels are present and one or other is specially enlarged and supplies the greater part of the limb but it is pointed out that in the forms where the two vessels co exist they anastomose with one another in the body wall at the base of the limb This anastomosis may be dissected out in lizards, where the dorsal vessel is specially enlarged and in crocodilian reptiles, where the ventral artery plays the important part

In the higher forms (birds and mammals) one of the arteries alone is present, but the cetacean group of mammals forms an exception to this rule In this group both arteries are to be found and it is the ventral, not the dorsal as in mammals generally, which specially supplies the limb

With reference to the development of the carotid artery in birds, Rathke believed that the external carotid was the prolongation of the ventral trunk from the extremity of the third arch towards the head, and regarded the branches of the external carotid as derivatives of this ventral vessel The internal carotid he looked upon as representing the third arch and its dorsal continuation towards the head, while the common carotid was believed by him to be the portion of the ventral vessel between the third and fourth arches

It is pointed out that if the observations already explained as to the origin of the subclavian artery be accepted, and that vessel be held to arise from the ventral extremity of the third arch, then, if Rathke's theory of the external carotid be true, the subclavian should be found in the adult as a branch of the external carotid, but this is not the case The common carotid artery, which Rathke regarded as a ventral vessel, runs towards the head in the adult bird, upon the dorsal aspect of the alimentary canal, and distributes intervertebral branches as it goes Rathke believed that this artery was originally ventral in position and passed gradually round the œsophagus in the course of growth until it finally reached the dorsal aspect, but the author points out that while the artery does change its position somewhat, it is to a much more limited extent than Rathke believed In the chick of the third or fourth day the main vessel for the supply of the head is the dorsal continuation from the third arch the ventral vessel being small The examination of the further development by sections and dissections makes it evident that the ventral vessel dwindles in importance, and becomes finally a small branch passing from the subclavian to the ventral aspect of the trachea, while the dorsal prolongation becomes the sole supply of the head At the close of the sixth day the dorsal connexion between the ends of the third and fourth arches is still present, and it is continuous with the common carotid, which may be easily followed to the head as a dorsal vessel, and its external branches, as well as its internal are therefore to be regarded as derivatives of a dorsal stem In the adult the common carotid lies in the middle line of the neck in contact with its fellow of the opposite side, but in the embryo the vessels of opposite sides are at some little distance from one another by the seventh day, however they have approached one another so as to be almost in contact This change, however, is not, as Rathke supposed the passage of a ventral vessel to a dorsal position but a slight alteration in the line of a vessel already dorsal

When the carotid system of different groups of vertebrate animals is examined, it is found that in those forms where on account of the preservation of the dorsal connexion between the third and fourth arches the continuous dorsal longitudinal vessel can be traced, the branches for the supply of both internal and external aspects of the head arise from this dorsal vessel In these forms the ventral prolongation supplies only the tongue This is the case in most lizards As the higher stages are reached the disappearance of the portion of the dorsal vessel between the ends of the third and fourth arches makes a comparison of the vessels uncertain but the author has discovered in the crocodile, and as an abnormality in a guillemot, solid cords stretching between the common carotids and aortæ on the dorsal aspect of the alimentary canal In these cases it is seen that the greater part of the common carotid is to be regarded as the dorsal prolongation from the third arch towards the head The ventral vessel in birds and crocodiles is not, therefore, the external carotid, as Rathke has it, but an artery running upon the trachea and supplying branches to the muscles on the ventral surface of the neck

XXIII. "On Radiation from Dull and Bright Surfaces." By J. T. BOTTOMLEY, M.A, F.R.S.E. Communicated by Sir W. THOMSON, Knt., F.R.S. Received May 26, 1887.

In connection with an investigation on heat radiation which I have been carrying on for some time past, and on which I recently presented a communication to the Royal Society, I have had occasion to examine the important results obtained by Mr. Mortimer Evans on the radiation of light and heat from bright and dull surfaces when incandescent ('Roy. Soc. Proc.,' vol. 40, 1886, p 207), and I have repeated and verified some of his experiments. Mr Evans experimented on carbon filaments of incandescent lamps; and in calculating, for my own use, the resistances of the filaments at different degrees of incandescence I was led to an unexpected result, and hence to an investigation of which I desire just now to offer a preliminary notice.

In order to explain, it is necessary for me to state briefly the object and nature of Mr. Evans' experiments. Their object was the comparison of the radiation from surfaces having a bright, polished appearance with that from dull surfaces having the appearance of lampblack; and he was led to an important practical conclusion as to the superior light-giving efficiency of the brilliant-looking filament. For these comparisons the *same* filament was treated in such ways as to alter the surface from dull to bright and back again. It was taken out of the glass globe for the purpose, and after treatment placed in a fresh globe, which was then exhausted. The lamps thus constructed and reconstructed were tested at various candle-powers, the energy for each candle-power being determined.

The tables given in Mr. Evans' paper show for the filaments in different conditions the potential and the current required to maintain different candle-powers from four candles upward. Using his numbers, and supposing Ohm's* law to hold for the carbon filaments, I have calculated the resistances of the filaments at different candle-powers.

Two filaments used by Mr. Evans afforded satisfactory data for my calculations. They are designated in his paper D, DD, DDD, and C, CC, CCC. They had been treated in the following manner:—The filaments D and C were "flashed" so as to have a dull surface with the appearance of lampblack. DD and CC are the same filaments flashed so as to have a brilliant surface, which, though black, has something of the appearance of frosted silver. DDD, CCC, are the same filaments again rendered dull as at first. The following table shows the volts, amperes, and calculated resistances at the candle-powers given in the left-hand column —

---

* I have already commenced an investigation into the question of the conformity of carbon filaments at different temperatures with Ohm's law.

Table I —Carbon D

	D			DD			DDD		
Candles	Volts	Amp	Resist	Volts	Amp	Resist	Volts	Amp	Resist
4	46 5	1 02	45 6	37 3	1	37 3	34	1 28	26 6
10	52 5	1 20	43 8	42	1 13	37 17	38 5	1 52	25 3
20	58 3	1 40	41	47 8	1 32	36 20	43	1 77	24 3
40	65	1 62	40 1	52 5	1 53	34 3	48	2 06	23 3
50	68	1 70	40 0	54 2	1 60	33 9	50	2 12	23 6*

Table II —Carbon C

	C			CC			CCC		
Candles	Volts	Amp	Resist	Volts	Amp	Resist	Volts	Amp	Resist
4	45	0 86	52 33	34	0 95	35 79	30	1 16	33 62
10	56	1 12	49 99	39	1 12	34 82	44 5	1 38	32 24
20	62	1 28	48 43	44	1 28	34 37	40 5	1 53	32 37*

Now if we suppose the resistance of the carbon filament to depend on the temperature the resistance diminishing as the temperature increases (though very probably not in simple proportion) and if (as we should do in the case of a metallic wire) we use these resistances in order to compare the temperatures of the filaments at different candle powers we are led to a remarkable result Taking the filament D and dividing the resistances of D DD and DDD at four candles by those at the higher candle powers we obtain numbers which may be looked on as ratios of conductances and which may be taken as indicating though not exactly representing corresponding changes in the temperature of the carbon

Table III

To pass from—		D	DD	DDD
4 candles to 10 candles	{increase of conductance is} from 1 to	1 041	1 006	1 047
4 20		1 096	1 028	1 095
4 40		1 137	1 087	1 137
4 50		1 14	1 100	—

---

* The two results which appear last in these tables seem anomalous and therefore I have not used them in my calculations

The filament C gives confirmatory results, but unfortunately it seems to have broken down early in the condition CCC. I find, however,—

Table IV.

	C.	CC.	CCC.
To pass from—			
4 candles to 10 candles {increase of conductance is} from 1 to	1 047	1·028	1 043
4 " 20 " " "	1·081	1 041	—

Comparing these numbers we are led to the result that, if we admit the assumptions I have made, the temperature to which the carbon must be raised in order that it may give out light of a definite candle-power is higher when the surface is in the dull condition than when it is in a brilliant metallic-looking state.

This result was to me so unexpected that I proceeded to test it directly by the following experiments:—Two glass tubes, similar in every respect, were constructed, containing two precisely similar platinum wires cut from the same hank, which had been specially drawn for me some months before by Messrs. Matthey and Johnson. One of the wires was in its natural bright condition, while the other was covered with the thinnest possible coating of lampblack, which was put on by passing the wire quickly and steadily through the flame of a paraffin lamp. The construction of these tubes is shown in fig. 1. The platinum wire *ab* is kept stretched by two spiral

Fig. 1.

springs of copper, being silver-soldered to two extremities of these springs. Two loops *l* at the other extremities of the spirals pass over two pieces of glass rod *gg*, *gg*, which are passed in by side tubes, blown on to the main glass tube; and the spirals pull on the glass rods. The ends of the side tubes are sealed up after the glass rods are in their places, with the exception of one, which is used for connecting to the Sprengel pump, and is finally sealed when a complete vacuum has been made Flexible copper electrodes *ee* are silver soldered to the loops at the ends of the copper springs and to multiple platinum wires which are sealed into the glass tube at A and B Fine platinum wires *pp* are attached to the main wire and are brought through the sides of the glass tube and these serve as potential testing electrodes The two tubes and their fittings are as has been said perfectly similar in every respect except that one platinum wire is covered with an extremely thin coating of lamp black

The two tubes were attached to a glass fork and were simultaneously exhausted with the Sprengel pump down to about two millionths of an atmosphere all the well understood precautions as to drying &c being carefully attended to and they were then at the same moment sealed off from the pump The length of the tubes AB is 22 inches over all and the internal diameter of the tubes $\frac{7}{8}$ inch The distance between the potential electrodes *pp* is 15 inches (38 1 centimetres) The diameter of the platinum wire *al* is 0 022 inch (0 0599 centimetre) On testing the resistance of the two platinums between the potential electrodes *cold* and at the same temperature it was found to be the same for both to less than one one thousandth part of the resistance of either of them

The tubes having been prepared as described above they were connected in parallel arc to a battery of six secondary cells in series a variable platinoid wire being added in series with the tube containing the bright platinum in order to regulate its current and a rheostat designed for carrying strong currents was used to control the whole The connexions will be readily understood from a glance at fig 2

FIG 2

With the rheostat and the variable platinoid wire the two platinums were then brought to the same incandescence (as judged by the eye) at various brightnesses from just visible redness up to nearly white heat, and the resistances of the platinums between the potential

electrodes were measured by means of a high resistance reflecting galvanometer, suitably arranged, with shunt and interposed resistance, for the purpose in hand

The result of my experiments is to bear out completely the deduction which I had made from Mr. Mortimer Evans' numbers; and to show that the temperature which produces, for example, the appearance of a certain red heat, is very much higher when the surface of the heated body is dulled than when it is bright as in a polished metal. I am not yet prepared to give a definite numerical comparison; but in order to show that the difference of temperatures referred to amounts to many degrees of temperature, I may be allowed to give the following statement.

The two wires being at the same dull red heat, which from previous experience I estimate at perhaps 600° C., in the case of the bright-surfaced wire, the ratio of the resistance of the lamp-blacked platinum to the bright platinum was 130 : 93. Platinums differ very much as to variation of resistance with temperature; but in most specimens the resistance is doubled, when the temperature is raised from 0° C. to a temperature of from 300° C to 400° C.; and for any particular platinum wire the change in resistance is almost in simple proportion to the change in temperature From this statement it may be judged that the difference of temperatures between the two platinums, dull and bright, when giving out the same light, was a great many degrees centigrade.

The difference of temperatures of the two glass envelopes was also very striking. The glass tube containing the bright wire was not even unpleasantly warm; while in the case of the other it was so hot as to blister the skin of the hand; and in this connection it is to be remembere[illegible]hat the vacuum in the two tubes was the same.

I propose as soon as possible to continue this investigation and render it more complete.

XXIV. "Note to a Paper on the Blood-vessels of *Mustelus Antarcticus* ('Phil. Trans.,' 1886)." By T. JEFFERY PARKER, B.Sc. Lond., Professor of Biology in the University of Otago. Communicated by Professor M. FOSTER, Sec. R.S. Received May 2, 1887.

My attention has been called by a perusal of Professor Milnes Marshall and Mr. C. H. Hurst's 'Practical Zoology' (London, 1887), to an omission in my description of the venous system. These authors describe and figure, in *Scyllium canicula* (pp. 218 and 224) a transverse anastomosis, the *inter-orbital sinus*, connecting the right and left orbital sinuses, and running in the floor of the skull immediately caudad of the pituitary fossa

I find that this anastomotic trunk is present in *Mustelus antarcticus*, in which species, however, it hardly deserves the name of sinus, being only 1 mm in diameter in a dog fish 1 metre long Its median portion is situated, not in the actual cartilage of the skull floor, but in the thick perichondrium of the pituitary fossa, where it lies immediately dorsad and caudad of the arterial commissures *w* (fig 6, Plate 35) at their point of crossing Passing laterad on either side it pierces the cartilage of the cranial floor, and finally enters the orbit by an aperture placed just cephalad of the trigeminal foramen, and about 5 mm caudad of the carotid foramen

I doubt whether this can be the anastomotic trunk described by Robin (see p 712), since it is not situated "derrière les orbites ' and can hardly be described as " un sinus plus ou moins vaste

The vessel in question ought to have been shown in the diagram, fig B (p 723) as a narrow trunk connecting the orbital sinuses (*orbit s*), and should have been referred to in the general account of venous anastomoses on p 722

XXV ' On Rigor Mortis in Fish, and its Relation to Putrefaction

By J C. Ewart, M D, Regius Professor of Natural History, University of Edinburgh

Communicated by J. Burdon Sanderson, F R S Received June 6, 1887

1 *The Nature of Rigor Mortis*

It has been long recognised] that rigor varies extremely not only in the time of its appearance, but also in its intensity It may be well marked and resemble closely a spasm, or so indistinct that it is better compared to a stiffening than to a contraction of the muscles So much is this the case that it might be convenient to describe rigor as accompanied with contraction in some cases and with stiffening in others I have often noticed that when rigor comes on immediately after the loss of muscular irritability, it looks extremely like contraction, but when it is postponed for days, by lowering the temperature or otherwise, it more closely resembles coagulation I am inclined to believe that whether the rigor resembles a contraction or a mere stiffening depends on the condition of the nervous system If the coagulation of the myosin takes place at or about the same time as the death of the nerves, the rigor will to a certain extent be physiological, and simulate a contraction in the extension of the fins, the bending of the trunk, &c , whereas if the coagulation only sets in some hours, or it may be days, after the death of the nerves, the rigor will be purely pathological, and consist of a mere fixing of the muscles in whatever position they happen to be. Another important consideration is what determines the time of appearance and strength of the rigor. In some instances I have been unable to detect rigor, in others, it has appeared at ordinary temperatures, a few minutes after death, while in other cases it appeared from ten to twenty hours after death. Again in some cases it is extremely weak and of short duration, whilst in others it is well marked and prolonged. It may be safely asserted that if all the nerves in a given muscle were destroyed, that muscle would still pass into rigor. But although the rigor would probably set in were all the nervous elements destroyed, the nervous system has apparently considerable influence in determining the time of appearance of rigor. Some physiologists seem to believe that the rigor comes on when and only when death has reached the muscles, by travelling in some cases hurriedly, in others slowly, from the central nervous system along the motor nerves. I hope to show that the longer the central nervous system continues to act, not only will the muscles sooner die, but the rigor will be the weaker and shorter, though in some cases from the arching of the trunk and extension of the fins, it may appear to be otherwise.

Let us suppose that two fish are instantaneously killed, the one in a vigorous, the other in an exhausted condition. In the former a considerable time will elapse before the energy of the muscles is exhausted, before the explosive material is all used up, while in the latter the muscles having already expended nearly all their energy during life, and little or no new productive material having been formed after death, they will soon die. Further, in the fish killed in an active condition, the muscles will give rise to a well-marked lasting rigor, whilst in the other it will be weak and of short duration. The result of artificial exhaustion is the same as that of natural. If a rabbit is killed and immediately after death the muscles of one hind limb exhausted by an interrupted current, rigor sets in in the exhausted limb two to three hours sooner than in the other. In the same way, if a fish is tetanised immediately after death, rigor sets in quicker than in another fish which has escaped stimulation. But further, if two fish are killed and the central nervous system at once destroyed in one, but left intact in the other, rigor will be considerably later in appearing in the pithed fish. The explanation possibly may be that the central nervous system after death tends to exhaust the latent energy of the muscles by constantly stimulating them into action; while, on the other hand, when the central nervous system is destroyed, the muscles are not stimulated into action, and therefore their final passage into rigor depends chiefly on the temperature and other surroundings.

2 *The Ordinary Phenomena of Rigor in Fish*

The changes which take place before and during rigor will be best illustrated by the following experiments —

(1) Physiological Laboratory Oxford 4th February 1887, 3 P M —A large active perch (*Perca fluviatilis*) was taken from the tank and laid on the floor of the Laboratory temperature 48° F For about twenty minutes the perch at irregular intervals was very active but the movements gradually diminished and about thirty five minutes after it left the water all movements had ceased and there was no response to mechanical stimulation When stimulated at 3 45 by induction shocks there was no response until the secondary coil indicated 15 cm * The muscular irritability gradually diminished and at 5 45 there was only a slight response at the break with the secondary coil at 0 cm At 5 50 the lower jaw and gill covers were nearly rigid, and the irritability had quite gone in the muscles near the root of the tail the last to survive At 6 0 the pectoral and dorsal fins were rigid and at 6 10 the rigor had extended as far as the pelvic fins At 6 15 the whole fish had passed into a pronounced rigor, the mouth was open the gill covers projected outwards, all the fins were extended and owing to the shortening of the muscles of the left side, the fish (which was 9 inches in length) was sufficiently curved to form an arc of a circle 30 inches in diameter While the muscles remained irritable they were neutral or amphichroic but as the rigor extended from before backwards they became distinctly acid

As soon as the rigor had set in the perch was placed under a bell-jar in a porcelain dish containing sufficient water to keep the skin moist At 10 P M the rigor was still well marked, but next morning (5th February) at 10 A M the rigor had disappeared from the lower jaw, gill covers, and pectoral fins When placed with the convex side looking upwards, the lateral curvature of the trunk soon gave way and at 12 noon the whole fish, except about 3 inches at the tail end, was quite limp

At 10 30 A M, the muscles in front of the dorsal fin were neutral, those behind distinctly acid, at 12 noon the muscles of the anterior half were slightly alkaline those near the root of the tail were still neutral Numerous bacteria were found in the layer of muscles lying around the body-cavity, and a few were found in the muscles under the skin in front of the dorsal fin, but no bacteria could be discovered either by direct observation or by cultivation in the muscles near the root of the tail

On the 6th February putrid odours were discernible, all the muscles were getting soft, and bacteria, plentiful in the muscles around the body-cavity, were extending into the caudal region

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* A single Daniell was used in the primary coil in the Oxford and two Smees in the Edinburgh experiments

In this case death occurred about 35 minutes after the fish was taken from the water muscular irritability disappeared and rigor began to appear 2 hours 15 minutes after death, the rigor was completed in 25 minutes after it set in, and it had vanished about 21 hours after death

(2) Zoological Laboratory, Edinburgh, 25th March, 10 A M —A common eel (*Anguilla vulgaris*) 18 inches in length was killed by knocking on the head At 6 P M (8 hours after death) the whole trunk responded freely to mechanical stimulation and the heart was still beating At 10 A M of the 26th (24 hours after death) there was only a feeble response to mechanical stimulation, but strong contractions were produced when the electrodes from an induction coil were applied to the skin,—the secondary coil at 15 cm At 1 P M the muscular irritability had slightly diminished in the anterior third, at 6 P M it was still less marked, and at 10 A M of the 27th (48 hours after death), with the secondary coil at zero, the muscles of the anterior third contracted very slightly At 12 noon the muscles of the anterior 5 inches gave no response, but those of the middle third still contracted readily, and the muscular irritability increased towards the tail end Two hours afterwards (*i e*, 52 hours after death) the greater portion of the anterior third had become rigid—the rigor beginning in the lower jaw and passing backwards affecting the gill-covers and pectoral fins and then the muscles of the trunk At 4 P M the muscles of the anterior portion of the middle third of the eel no longer responded to electrical stimulation, and at 8 P M, the anterior half (about 9 inches in length) was rigid while the posterior half still responded when stimulated—the strength of the contractions still increasing from before backwards The muscles of the rigid half had a distinctly acid reaction, those of the posterior half were neutral or very faintly alkaline—a narrow zone near the centre being amphicroic The muscles of the anterior third immediately under the skin were neutral and contained no bacteria, but those next the peritoneum had a few bacilli and micrococci, and were alkaline in reaction

On the morning of the 28th (9 A M) the rigor had all but disappeared from the anterior third, the middle third was quite stiff, and the posterior third, except near the tail end, contracted very feebly with the secondary coil at zero

At 1 P M rigor had passed from the anterior half, but the muscles of the posterior third were still irritable The reaction of the muscles in the anterior third was now slightly alkaline, and they contained a few bacilli and micrococci similar to those found on the previous day in the muscles around the body-cavity At 5 P M only the terminal 3 inches responded when the electrodes were introduced into the muscles, and the rigor was passing from the remainder of the middle third and making its appearance in the front portion of the posterior third At 6 P M the rigor had all but gone from the middle third, and a weak rigor had set in in the posterior third At 8 P M (82 hours after death) the whole eel was quite limp—the rigor on the posterior third having been weak and of short duration The alkaline reaction increased from before backwards, to about 3 inches from the tip of the tail, where it was neutral, and bacteria could be detected in the muscles a little beyond the middle half Next morning (29th March) all the muscles were alkaline, and a few bacteria were present even in the muscles near the tail end, and the anterior portion was smelling slightly This eel was under observation until the 13th April, when putrefaction had considerably advanced While under observation the eel was kept in water which varied from 48—52° F I may add that the blood and peritoneal fluid were examined immediately after death, and that though small bacilli were fairly abundant in the lymph it was impossible to discover any organisms in the blood In this eel the muscular irritability lasted in some of the muscles for nearly eighty two hours after death As a contrast to this above experiment, I may describe shortly another

(3) On the 6th April, an eel, also about 18 inches in length, which was killed by an electrical shock from a Holtz machine, passed immediately into rigor—what might be called ' cataleptic rigor " The posterior half—in which there is no body cavity—was sterilised (by placing it for a short time in a 5 per cent solution of phenol) and then introduced with the usual antiseptic precautions into a jar of sterilised distilled water The rigor still (June 16th) continues on this portion (the posterior half) of the eel, while the anterior half, which was introduced into a 5 per cent solution of phenol *after the rigor had disappeared,* is now quite limp and soft

From these experiments it may be inferred that under ordinary conditions there is an intimate relation between loss of irritability and the setting in of rigor, and that rigor vanishes as the bacteria invade the tissues

3 *The Time at which Rigor appears*

Under ordinary circumstances the setting in of rigor in the various kinds of fish seems to depend on the amount of irritability of the muscles at death In all probability it might be possible to discover when the rigor would come on by determining the amount of free acid in the muscles, in other words, there is a relation between the appearance of the rigor and the amount of catabolic material in the muscles at death This seems to vary in an unaccountable way, *e g*, if three two-year-old trout of as nearly as possible the same size, which have been living under as nearly as possible identical conditions since the day of hatching, are captured at the same moment while lying quietly in the corner of a tank, and allowed to die in the landing net, the rigor may appear in one (A) 15 minutes after death, in another (B) 30 minutes, and in the third (C) 40 minutes after death We must suppose that this variation results either from the condition of the muscles at death, or from the influence of the nervous system

If at death the muscles of (C) contained (owing to the oxygenation of the blood continuing longer) less bye products than (A), it might be possible to understand why the time at which the rigor set in differed Again, if in (A) the nervous system continued to produce muscular contractions longer than in (C), *i e*, led to the more complete exhaustion of the muscles of (A) than (C), the difference might be easily understood It is of course difficult, if not impossible, to determine which (if either) of these explanations is the correct one, but that they may both have some influence in the result may be inferred from the following facts (1) If two trout are taken from the water at the same time, and one is left with its gills freely open in the landing net, while in the other the gills are kept firmly closed by an elastic band, the one with the gill-covers extended will die, it may be 20 minutes before the other,* but will be 30 to 40 minutes later in becoming rigid, the reason apparently being that the closed gill-covers prevent the evaporation from the gill-chamber, and the consequent increase in temperature and loss of function of the gill filaments (2) If two trout are taken from the water at the same time, the one allowed to die in the landing net, while the other is at once killed and pithed, the rigor sets in in the former several hours (4—8) sooner than in the latter, *i e*, it is later in appearing in the fish in which the brain has been destroyed

Although in some cases it is difficult to account for the time at which the rigor sets in, fairly satisfactory explanations can be given in others It is well known that most fish can live for months without food In fact fish in confinement often appear to "thrive" best when not fed, they are less sensitive and less liable to suffer from disease Even in a wild state fish seem to all but give up feeding for weeks at a time, more especially during the spawning season, and the chief difference between under-fed and well fed fish appears to be that in the former there is little or no growth, and the spawning period is delayed or the formation and maturation of the roe and milt are arrested But although many fish are capable of living for months without food in aquaria, now and then one sickens and dies without any apparent cause.

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* This was first pointed out to me by Sir James Maitland, Bart, when visiting the Howieton Fishery

As in warm-blooded animals which die before maturity is reached from wasting diseases, the rigor soon appears and as rapidly goes, so in young fish which have been living in confinement, the rigor is often weak and evanescent.

For example, a young roach about 6 inches in length, which for some hours had barely managed to survive was killed, and, though carefully watched, it was impossible to detect any rigor, and equally impossible fifteen minutes after death to obtain any response from electric stimulation with the secondary coil at zero. Again, as in a "hunted hare" the rigor sets in rapidly and is of short duration, so it is in a long "played" fish. On April 14th a trout was chased for nearly half an hour before it was landed. About 20 minutes after it was taken from the water, even although the brain was destroyed immediately after death, the muscular irritability had disappeared, and the rigor was complete before 30 minutes had elapsed with the temperature at 9.5° C. Under ordinary conditions, if an active two-year old trout (*S. levenensis*) about 9 inches in length is taken from the water and left in the landing net, it usually lies perfectly still, only giving an occasional wriggle. During the first few minutes the breathing movements are performed, but as soon as the fish realises fully it is out of the water, the mouth and gill-covers are tightly closed—an instinct which fish display, whereby they better their chance of surviving until they again perchance reach their native element. In from 20 to 30 minutes, probably owing to the muscles being exhausted, the mouth is opened and the gill covers are widely extended, and in a few minutes later (5—10) the fish dies. If it dies in about 25 minutes, the muscles will respond to mechanical stimulation 10 minutes after death, and 60 minutes after death all the muscles will respond freely to electrical stimulation with the secondary coil at 15 cm. Gradually the muscles from before backwards lose their irritability, and 1½ hours after death, though the muscles near the tail still respond with the secondary coil at 15 cm., the muscles of the lower jaw will only respond when the indicator is at zero, and 2 hours after death only the muscles of the posterior half of the trunk retain their irritability. At 2½ hours after death even the caudal muscles require the secondary coil at 12 cm., 10 minutes later at 8 cm., and in 15 to 20 minutes more (about 3 hours after death) only a faint response is obtained with the secondary coil at zero. Two hours after death—before muscular irritability has gone from the caudal muscles—the muscles of the jaw become rigid and the stiffening extends backwards, overtaking the gill covers, the pectoral, dorsal, and pelvic fins and one myotome after, until the rigor is complete.

The time required is never the same, but on an average the rigor is accomplished in a trout allowed to die in a landing net, and kept afterwards in the air at a temperature of 9° C., in from 1 to 1½ hours—3 to 3½ hours after death. When a trout is taken from the landing net immediately after all signs of life have gone, and placed in water at the same temperature (9°C) the irritability continues about 10 minutes longer, and the rigor is from 15 to 20 minutes later in setting in When the temperature is raised to 15° C. the irritability goes, and the rigor appears in from 20 to 30 minutes, and reaches the caudal muscles about 45 minutes after death. At a temperature of 25° C. the rigor may set in in 15 minutes, and be complete in about 25 minutes after death, while at a temperature of 30° rigor often comes on in the trout 5 minutes after death, and vanishes 15 or 20 minutes later. At a temperature of 38° C heat rigor at once sets in. As the temperature is lowered the rigor is later in making its appearance, and a considerable period elapses between the loss of muscular irritability and the setting in of rigidity. At low temperatures it is often extremely difficult to say at what time the stiffening begins. A trout in water at 1° C seemed to pass into rigor about 23 hours after death; at −1° C. there was no distinct rigor 30 hours after death, but well-marked stiffness 10 hours later, but at lower temperatures (−7° to −20° C.) neither rigor nor stiffening could be detected in four trout which had been respectively 2, 3, 4, and 5 days in the freezing mixture. Further, trout which had been subjected to a temperature below −7° C. never stiffened, even when introduced immediately after thawing into water at a temperature of 25° C

Judging from the above and other experiments, it seems that raising the temperature either before or after death has the same influence as muscular or nervous exhaustion in hastening the rigor. The increased temperature quickens all the chemical and other changes, and thus leads to the rapid and all but complete destruction of the catabolic material stored up in the muscles. On the other hand, cold either diminishes or arrests the metabolic changes. At a temperature below freezing point the muscles contract, even when stimulated less quickly, and hence they long retain almost unaltered the contraction-producing material which happens to be present when death sets in; so that when the rigor eventually appears it, as already mentioned, more resembles a mechanical coagulation of the muscles than a strong contraction. It is difficult to determine whether the rigor, which appears at a low temperature (5° to −1° C.), is really stronger than the rigor that comes on at a high temperature. When a trout, in which the rigor has set in at a temperature of 2° C., is placed in water at a temperature of 25° C., stiffening vanishes in about the same time as it would had the rigor set in at a temperature of 20° C.

The intensity and duration of the rigor which follows death in warm-blooded animals from lightning has been again and again discussed It is stated by some that rigor never appears, whilst others assert that the rigor which follows death by lightning is often well marked and of considerable duration From experiments made with fish it seems that in some cases the rigor may be instantaneous and well marked, or it may appear some time after death and be of short duration, whilst in others it may resemble closely the rigor that sets in after death from ordinary causes When a trout receives a sufficiently strong electric shock it is instantaneously killed, and at the same moment thrown into a well-marked tetanic spasm which passes directly and almost imperceptibly into rigor A trout about 10 inches in length which received a strong shock* by placing one electrode under the left gill cover and the other (a chain) round the tail, was thrown into a pronounced spasm which closely resembled a heat rigor, the lower jaw was depressed, the gill covers widely opened, the fins fully extended, and the trunk strongly arched After the shock the gill covers and tail quivered two or three times, and in about five minutes the muscles lost their irritability, and in three minutes more were strongly acid Ten minutes after the shock the fins became if possible more extended than before, this further extension indicating probably the passage of the tetanic spasm into a true rigor

In all the experiments when a sufficiently strong current was used the result was the same, but on several occasions, though the current was strong enough to cause death, the muscles recovered from the tetanic spasm and rigor set in about one hour and twenty minutes afterwards On other occasions, though the fish was thrown into a strong spasm and seemed dead, there was in from ten to fifteen minutes complete recovery When fish, which had not only been killed by the shock, but had apparently also passed into a strong rigor, received several additional shocks not later than thirty minutes after the first, the rigor disappeared, and although the muscles failed to recover their irritability, rigor did not again set in for nearly an hour This seems to confirm what has already been observed by others, that rigor up to a certain limit may be broken down and kept at bay for some time The breaking down of the rigor may be accounted for by supposing that only a certain proportion of the fibres had stiffened, or that coagulation had been incomplete It was especially noticed that when the muscles recovered from the first tetanic spasm in fish which had been killed, it was impossible to bring on a second spasm sufficiently strong to pass directly into rigor, and further that the spasm which appeared in fish stimulated after death was never so well marked as in fish which were simply under the influence of ether

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* A Holtz machine was used, and the jar was 8½ inches in diameter with coatings 18 inches high

Nevertheless, when both the brain and spinal cord had been destroyed, the spasm was sufficiently strong to pass directly into rigor, showing that however much the peripheral nerves were concerned, "cataleptic rigor" was possible without the central nervous system

It has always been extremely difficult to account for partial cataleptic rigors, such as sometimes occur in the battlefield from gunshot wounds It has been supposed by Falk and others that these obscure rigors result from injury of the spinal cord I had no difficulty in producing partial rigors in fish, *e g*, when one electrode was introduced into the brain of a fish, and another into the muscles half along the lateral line, the anterior half of the fish was thrown into strong rigor, from which there was no recovery while the rest of the fish remained quite limp for five hours after death Before the posterior half of the fish had become stiff, the rigor had all but disappeared from the anterior half the posterior half remaining rigid for nearly twelve hours In the same way, in a fish in which the brain and spinal cord had been destroyed the posterior half could be thrown into rigor which almost vanished before the anterior half became rigid Hence we may suppose that in partial rigor a strong tetanic spasm has been produced directly influencing the muscles, or by injury of the nerves or the nerve centres by which a particular group of muscles is controlled I may mention that in several instances the electric current was seen to flash along the surface of the skin, and that when this happened there was often marked pigmentation of the one side, while the other remained pale and further the muscles under the darkened skin were often quite rigid, while those under the unaltered skin remained for a time elastic and extensible

The influence of Faradaic currents and of often repeated continuous currents was very marked A trout, *e g*, in which after death first the brain and afterwards the spinal cord were stimulated, was thrown into spasms which became weaker and weaker until 10 minutes after death, no response was obtained In this case the rigor set in 20 minutes after death, and 20 minutes later it had extended to the caudal muscles The appearance and nature of the rigor were always directly related to the previous exhaustion produced in the muscles

The effect of animal electricity seems to correspond to that of ordinary electricity, except that in fish killed by electricity from electric organs the rigor seems later in setting in For example, in two small roach killed by the electric eel (*Gymnotus electricus*) in the insect house of the Zoological Gardens, London, on the 21st March, and kept under observation in water at a temperature of 45° F, no distinct rigor had set in twelve hours afterwards * Two roach killed on March 31st about 5 P M, after receiving numerous weak shocks from the apparently exhausted electric organs, began to stiffen 10 hours afterwards, and were quite rigid $11\frac{1}{2}$ hours afterwards, the temperature varying from 50—44° F On the 3rd April two other roach were "struck" when the fish was vigorous, one was killed by the first shock, the other after receiving two shocks, and both were quite rigid $5\frac{1}{2}$ hours afterwards We thus see that the setting in of the rigor is related to the strength of the shock received, but that even when the shock is strong enough to cause instantaneous death the rigor is delayed for several hours The advantages of being able to kill the fish instantaneously without producing immediate rigor is evident enough It would be often uncomfortable if not impossible for Gymnotus (and still more for the small-mouthed torpedo) to swallow a fish in strong rigor, and yet unless the fish were sufficiently "numbed" they would readily escape from their sluggish destroyers It may be mentioned that a trout which had been pithed immediately after death and placed in artificial gastric juice at a temperature of 100° F, became rigid in less than a minute, and the rigor completely disappeared 35 minutes later A similar fish in water at the same temperature became at once rigid, but the rigidity persisted for an hour and ten minutes

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* I am indebted to Mr Romanes for making this observation

Experiments were made to determine the influence of acid, alkaline, and other solutions in bringing on and keeping back rigor Salt solution, as is well known, prevents rigor setting in in proportion to its coming into contact with the muscles Alkaline and septic solutions seem to have no influence either way, while acid and corrosive solutions seem to hasten its appearance, but the latter only to a limited extent Generally speaking, whatever tended to influence the rigor influenced the irritability of the muscles, but at low temperatures there was no relation between the disappearance of the irritability and the setting in of the rigor The muscles lost beyond recovery their irritability in the trout when the temperature was kept for a few minutes at −70° C, but while this temperature was maintained no rigor appeared, and, as mentioned above, fish which were kept under −70° C for several days never became rigid when removed from the freezing mixture, in some cases, however, they seemed to become slightly firmer—the continued freezing probably so alters the tissues that the usual coagulation or stiffening is rendered impossible

4 *The Duration of Rigor.*

There is, as generally believed under ordinary circumstances, a close relation between the duration of a rigor and the time at which it sets in, *i e*, if a rigor sets in half an hour after death, it is not likely to last long, while if it appears twelve hours after death, there is a probability that it will continue for several hours On the other hand, there does not seem to be usually an intimate relation between the apparent intensity and the duration, for a short-lived rigor produced at a high temperature may look most pronounced, while a fish in a *strong* rigor (which may last for twenty-four hours or more) has often neither the gill-covers nor fins extended, nor the body distinctly arched. We must suppose that there is some relation between the duration of the rigor and the condition of the muscles when it sets in. If before the rigor appears the latent energy of the muscles has been all but exhausted, either before or after death, naturally or artificially, the rigor though well marked will be of short duration, while on the other hand if a considerable amount of rigor-producing material is left when the stiffening supervenes, the rigor, though not strikingly resembling a tetanic spasm, will be more intense and more persistent.

As the rigor comes on, all the muscles shorten (or contract), the extensors usually overcoming the flexors and the muscles of one side (or probably the red muscles of one side) overcoming the muscles of the other, and thus leading to arching or lateral curvature of the trunk. This curving is sometimes so intense that a fish 10 inches in length may form the arc of a circle little over 6 inches in diameter. Up to a certain time after the rigor sets in it may, either by electrical stimulation or mechanically, be broken down, not once, but several times; but the oftener the coming on of the rigor is interfered with, the final rigor is the weaker and the less persistent. Apparently at ordinary temperatures there is a regular order, not only in the stiffening of the various groups of muscles, but also of the various bundles of the individual muscles. If this is the case, the alternate appearance and disappearance of the rigor may, as already indicated, be accounted for by saying that when a partial rigor is broken down, the stiffening of certain muscles or portions of muscles has been arrested or destroyed, and when the rigor sets in again new muscles or muscular bundles have stiffened, while those in which the rigor had previously appeared either remain limp or have their rigidity completed. When rigor which has been fully established in isolated muscles or groups of muscles is destroyed, it never reappears. I have not yet succeeded in recording in a satisfactory manner the strength of the rigor under different conditions, but from the results already obtained, it appears the more rapidly the rigor comes on the more closely it resembles an ordinary muscle contraction.

A comparative table showing the changes which take place in the muscles of different animals at different temperatures while the rigor is coming on and going off would be very instructive. It may be mentioned that in fish, as in other vertebrates, as the rigor comes on there is a rise in temperature, and the reaction changes often rapidly from slightly alkaline or neutral to distinctly acid. In a trout about 10 inches in length the temperature during life was 10 15° C, the temperature of the water being 9° C, and the air 9 3° C After death the temperature fell to 9 5°, but as soon as the rigor had set in, the temperature of the muscles rose rapidly, reaching when the rigor was all but completed 10 3° C Ten minutes after the rigor had been completed the temperature was 10 2° and it gradually fell until it reached 9° C, forty five minutes after the maximum had been reached In this fish rigor began to disappear two hours after death, and as the rigidity vanished the temperature again rose, being 10° C three hours after death, 10 05° when the rigor four hours after death, had vanished from the anterior third of the fish, and 10 03° when only the muscles of the tail continued rigid, when the fish became quite limp, six hours after death the temperature was again 9° C As putrefaction proceeded the temperature again rose to 10° C, and it varied between 9 5° C and 10° C for two days, after which it was the same as the temperature of the laboratory The acidity increased as the rigor came on, and then gradually diminished until the muscles were neutral As putrefaction advanced the tissues became decidedly alkaline

The rigor is least persistent in fish which die in an exhausted condition at a high temperature If an exhausted trout is placed immediately after death in water at a temperature of 35° C, rigor appears in from 3 to 10 minutes and disappears in from 15 to 30 minutes, *i e*, at the most 40 minutes after death

If a trout is killed and pithed and then introduced into water at a temperature of 35° C, the rigor appears in from 10 to 15 minutes, and persists from 50 minutes to 1 hour In a fish treated in the same way at a temperature of 25° C, rigor appears in from 60 to 65 minutes, and persists for 2½ to 3 hours, while a similar trout placed in water at 15° C does not begin to stiffen for 5 hours, and the rigor may only be completed 7 hours after death, and begin to pass off about 20 hours after death The nervous system has doubtless considerable influence in determining the length of the rigor, as it has in deciding its time of setting in If an active trout (A) is carefully captured and allowed to die in the landing net, which when undisturbed it usually does without a struggle, and if when all signs of life in (A) have vanished, a second trout (B) is secured and at once killed and the brain and spinal cord destroyed, in (A) the rigor may begin to disappear 2 hours after it has been completed and may only last altogether 7 or 8 hours, while in (B) it may last at least 24 hours When the brain only is destroyed it disappears from 1 to 1½ hours sooner, the temperature being from 9—10° C

The difference of the duration of the rigor in fish which are allowed to die and in fish which are knocked on the head or have both brain and spinal cord destroyed is well marked at all temperatures above

5° C, but at temperatures below 5° C the difference is less evident, the cold serving either to paralyse the nervous system or to prevent the muscles responding to the weak stimulations which reach them

If two fish, one (A) with brain and cord destroyed, the other (B) with both intact (B having been allowed to die slowly in the net), are placed in water at a temperature of 25° C, the rigor in (A) persists for $2\frac{1}{2}$ to 3 hours while in (B) it vanishes in $1\frac{1}{2}$ to 2 hours If two similar fish are kept under observation at a temperature of 5° C, in the pithed specimen the rigor may last three days, while in the other it may not last 48 hours As it is impossible to suppose any vital changes take place after the rigor appears, its duration must depend on the condition the muscles are in when they become rigid, so that we must account for rigor in pithed fish persisting longer than in fish allowed to die naturally, in the same way as we account for rigor setting in at different times in fish differently treated

There is little to add to what has been already said as to the influence of temperature in driving off or maintaining rigor In unpithed fish the rigor at a temperature of 35° C may only last 30 minutes, at 25° C it may last 5 hours at 15° C it may persist for 24 hours, at 10° C 36 hours, at 5° C 46 hours at 1° C three days At −2° C it continues unchanged for an indefinite time On the other hand, in fish which after the rigor had set in were kept for several days at a temperature between −7° C and −20° C, the rigor disappeared before the thawing was completed

Perhaps the rigor was destroyed by alterations produced in the muscular fibres during freezing It was certainly not owing to the direct contact of the salt and ice freezing mixture, for the same results were obtained when fish were frozen in air and in fresh water in stoppered bottles That the rigor persists until the fish are thawed does not seem probable, because it persists for some time after thawing in fish which have been for ten days at −2° C and true rigor never appears in fish which have been kept for several days below −7° C

The duration of rigor which occurs after death from an electric shock varies considerably, the variation evidently depending either on the direct influence the charge has had on the muscles or on the condition of the central nervous system after the shock In a trout which was killed and thrown into instantaneous cataleptic rigor at one and the same moment, the rigor began to disappear from the jaw and gill covers $7\frac{1}{2}$ hours afterwards, and 16 hours later it had completely passed off In another trout in which only the anterior half was stimulated, the rigor had passed off 9 hours afterwards, about 3 hours after the rigor appeared in the posterior unstimulated portion Conversely when the shock was passed through the posterior half of a trout, the rigor continued until $8\frac{1}{2}$ hours after death, while the rigor in the anterior half, which was completed 6 hours after death, lasted 11 hours, the order of appearance and disappearance in this case being practically reversed

In a trout, in which one electrode was in contact with the skin while the other was in the water, the fish was thrown into a strong spasm, but not instantaneously killed The skin of both sides behind the point of contact of the electrode (which was near the head) became deeply pigmented, the jaws and gill covers continued to move at intervals for 30 minutes, after which there were no signs of life The tetanic spasm seemed to pass off to a certain extent, but as soon as the gill covers stopped acting the muscular irritability was lost, and a strong rigor set in which lasted for nearly 15 hours

Another trout, which was tetanised and killed by an electric shock, recovered from the spasm and passed into a rigor 2½ hours afterwards, which only lasted 7 hours

Another trout, which was killed, but not permanently tetanised by the first shock received seven other shocks, some of them of great intensity In less than an hour after the first shock rigor set in, but it was of short duration, for in 20 minutes after the stiffening appeared the jaw and gill covers were relaxed, and the whole fish was soft and limp 1½ hours after death

It may be taken for granted, from the experiments made, that the prolonged rigor of pithed fish is closely related with the destruction of the central nervous system, but it does not necessarily follow that the strength of the rigor in fish, instantaneously killed and stiffened by an electric shock has the same explanation It is doubtless possible that a single strong shock may, by destroying the nervous apparatus, produce the same results as pithing, but it is also possible that the appearance and duration of the rigor in fish killed by electricity may be largely influenced by chemical changes in the muscles

The observations made as to the appearance and duration of rigor in the trout have been confirmed by control experiments on other fish It will be sufficient in the meantime, to refer to the behaviour of the perch (*Perca fluviatilis*), roach (*Leuciscus rutilus*), and eel (*Anguilla vulgaris*), under conditions similar to some of those above mentioned

In a roach, which died in an exhausted condition, there was no response to mechanical stimulation, and 30 minutes after death only a weak response at break, with the secondary coil at 0 0 cm It was almost impossible to say either when the rigor set in or disappeared, and four hours after death the muscles of the trunk were alkaline, and contained bacteria A similar roach, which was killed by a blow on the head and afterwards pithed, responded freely to mechanical stimulation for 1 hour and 20 minutes At first the movements were vigorous, *e.g.*, when held in the vertical position half an hour after death, all the swimming movements were repeated. At 1 hour and 40 minutes after death the muscles responded with the secondary coil at 15 cm., and they still responded 9 hours afterwards with the secondary coil at 0·0 cm. Soon after the irritability of the muscles was lost, rigor set in, and extended slowly backwards without producing very marked extension of the fins, and was completed about 19 hours after death. The rigor persisted unaltered for 11 hours, when it slowly vanished in the same order as it appeared, leaving the anterior two-thirds of the fish quite flexible about 36 hours after death, but remaining in the posterior third for nearly 7 hours longer. The temperature in both cases varied from 11° C. to 12° C.

When a somewhat exhausted roach was placed in water at a temperature of 35° C., the muscles rapidly lost their irritability and the rigor set in 30 minutes after death, was well established in 45 minutes, and was disappearing 2 hours after death, and quite off 2 hours 45 minutes after death.

At a temperature of 20° C. the rigor set in 1 hour and 30 minutes after death, and had all but passed off 4 hours after death. At a temperature of −1° C. the rigor came on extremely slowly. On placing several roach in water at a temperature of −1° C. and taking them out at intervals, I came to the conclusion that in small roach the rigor set in between 24 and 27 hours after death. In roach which had been in water at a temperature of −1° C for 9 days, the rigor persisted for several hours after thawing, even when this was done very slowly. In the perch, generally speaking, the irritability of the muscles lasts longer the later the rigor is in appearing; *e g*, in a perch about 11 inches in length, which had been killed and pithed, the muscles were slightly irritable 13 hours after death, and the rigor was only fully established 9 hours later, and it had only disappeared from the anterior two-thirds of the fish 48 hours after death. But even a vigorous perch, if allowed to die in the usual way, may lose its muscular irritability and pass into rigor 2½ hours after death, and become flexible again 16—18 hours after death, at a temperature of 11·5° C.

5. *The Disappearance of Rigor.*

It has long been admitted that there is some relation between the disappearance of rigor and the beginning of putrefaction, that in fact putrefaction assists in driving away the rigor. While endeavouring to discover a simple means for preserving fish in a fresh condition last autumn, it occurred to me that there might be a closer relation between rigor and putrefaction than had hitherto been determined, and that it might in fact be possible to prevent putrefaction by maintaining the post-mortem rigidity of the muscles In order to ascertain what ground there was for entertaining this notion, I proceeded to study the origin and nature of rigor in fish and other animals At a very early stage I learned that the longer the rigor lasted the longer putrefaction was delayed, and also that putrefaction set in quicker in fish in which there was a large amount of putrescible matter in the alimentary canal, than in fish in which the alimentary canal was practically empty For studying the relation of bacteria to the disappearance of rigor, I at the outset used sea fish, but afterwards trusted chiefly to fresh water forms, owing to the great difficulty in obtaining haddock and other fish in a perfectly vigorous condition I soon observed that haddocks and whiting which were knocked on the head when captured (after the manner long practised by the fishermen who send "live" cod to Billingsgate) were longer in stiffening than fish which were left wriggling as long as their energy lasted in the hold of the boat Later I found that the rigor persisted longer in haddocks and whiting which were gutted immediately after capture than in ungutted fish If, *e g*, we take six haddocks (about the same size and captured on the same fishing bank at or about the same time) and, (1) leave two (Series A) to die in the usual way (temperature 7—8° C, (2) kill two (Series B) by knocking on the head and then pith, and (3) kill the other two (Series C) and both pith and gut, in Series A the rigor may appear 10 minutes after death, but in B and C it may not set in for 2 or 3 hours Further, in A the rigor may have disappeared from the trunk (the portion co-extensive with the body-cavity) 10 hours after death, while in B it may persist for 13 hours, and in C for 20 hours, and while A might be quite limp 21 hours after death, B might continue rigid for 25 hours and C for 30 hours after death

It may be taken for granted that rigor results chiefly from the formation and coagulation of myosin, and further, that the intensity of rigor depends on the amount of myosin formed in the various muscles If nearly all the myosin-forming material has been used up before rigor appears in producing muscular contractions, comparatively little myosin will be formed, while in fish which have been pithed (if pithing diminishes the muscular contractions) there will be (except when the rigor is greatly delayed, as in the tail of the eel) sufficient material left to admit of a considerable amount of myosin appearing in the substance of the muscles

But after all the amount of myosin formed in any given muscle accounts rather for the *firmness* of the muscle during the rigor than for the *duration* of the rigidity Why does the myosin not continue unchanged? Why does it not, as it were, liquefy? And why does the rigor persist not only longer in some fish than in others, but also longer in some parts of individual fish than in others?

Experiments show that the rigidity is easily overcome (1) by alternate flexion and extension; (2) by raising the temperature; (3) by freezing; (4) by the action of acids and alkalies; and (5) by means of organisms. That this last cause is more important than all the others put together might, perhaps, be inferred from the fact that in fish in which bacteria abound in the tissues at death, either no rigor or a very weak one makes its appearance. This inference is confirmed by the following observations and experiments. (1.) A septic solution was injected into the right femoral artery of a newly killed rabbit, the rigor, though it appeared about the same time in the right limb as in the left, disappeared much quicker from the right (2.) A large roach was killed on the 4th February and at once gutted. The muscles of the right side, immediately in contact with the peritoneal lining, were inoculated by septic bacteria (introduced by pricking the peritoneum with a rosette of needles fixed in a piece of sealing-wax) and the peritoneum covering the left side of the body-cavity was washed with a solution of corrosive sublimate (1 in 10,000) The rigor was to my surprise equally well marked on the two sides; but 24 hours after death, when the rigor began to disappear, the right side became limp about 2½ hours before the left, and 36 hours after death the muscles of the right side were soft and beginning to putrefy, while those of the left were still firm; further, a piece of muscle taken from under the skin of the right side opposite the anterior margin of the dorsal fin swarmed with bacteria, while a piece of muscle from a corresponding point on the left side contained comparatively few bacteria. (3.) Roach and trout, which were gutted immediately after death, and dipped for a time in solutions of phenol (5 per cent.) and corrosive sublimate (1 in 1,000), and afterwards introduced into sterilised water, retained their rigor unimpaired for an indefinite time. Whenever the fish, however, were transferred from the sterilised into ordinary water, rigor began to disappear—the passing off being always accelerated when organisms were introduced into the water, or when the temperature was raised. Believing the rigor in the above fish might be mere stiffening produced and maintained by the action of the phenol and corrosive sublimate solutions, I introduced fish from which the rigor had been driven off by heat into similar solutions of phenol and corrosive sublimate. Limp fish treated in this way never became stiff—the natural firmness of the fresh muscles was simply maintained. (4.) Eels which were thrown into instantaneous rigor by strong electric shocks behaved in the same way. The posterior half of a large eel (the part behind the body-cavity) in cataleptic rigor was placed in phenol and then in sterilised water on the 8th April. A similar portion was introduced on the same day into sterilised water after the skin had been rendered aseptic by corrosive sublimate.

Both specimens are still (June 16th) in well marked rigor, and in the muscles near the cut surface I have been unable to detect any bacteria, and the reaction of the muscles is still slightly acid.

These and other experiments justify the conclusion that rigor, under ordinary circumstances is in all probability driven away by putrefactive organisms. One of the most remarkable changes which accompanies the disappearance of rigor is the change of reaction of the muscles from acid to alkaline. As soon as the rigor begins to lose its hold the acidity diminishes, and gradually or rapidly disappears; for a time the muscles are neutral but sooner or later they are distinctly alkaline. The muscles around the body cavity become alkaline first, from these muscles the alkalinity extends outwards towards the skin and later extends into the muscles of the tail but in some cases a long interval elapses between the appearance of the alkaline reaction in the sub peritoneal muscles and the myotomes situated behind the body cavity. A point of considerable interest is that the reaction of the muscles under the skin passes from acid to alkaline before they are invaded by bacteria. This can be readily proved by introducing into a culture medium a fragment of muscle from under the skin of the trunk from which the rigor has just gone and which is already faintly alkaline and as a test experiment a similar fragment from under the peritoneum from a point as nearly as possible opposite where the first was taken. In the latter case bacteria rapidly appear, while no bacteria (if all the necessary conditions have been observed) will appear in the former. By a series of experiments I have proved that while weak solutions of hydrochloric and sulphuric acids are incapable of preventing the putrefaction of fish, they have the power of arresting or at least greatly retarding the development of ordinary bacteria. Seeing that the alkaline wave radiates from around the body cavity in advance of the bacteria, it is extremely likely that the one results from the presence of the other. In fact we may, until further experiments have been made, suppose that rigor disappears in the presence of a species of fermentation, that the bacteria which reach the tissues from the body cavity manufacture ferment like substances, which as they diffuse through the muscles drive the rigor before them, adapting the tissues on the way for the suitable reception and nourishment of a crop of putrefactive bacteria in much the same way as the husbandman breaks up and otherwise prepares the soil before sowing his corn. In all probability the duration of the rigor partly depends on the readiness with which the tissues can be made alkaline, and partly on the amount of mechanical obstruction the bacteria have to overcome in the muscular fibres. It is well known that gelatine and other culture-media, when slightly acid, or when too much dried, are rendered for a time altogether unsuitable for the cultivation of certain bacteria. In the same way the muscles of certain fish, either because of their peculiar chemical composition, or because of the peculiar disposition and structure of the tissues composing them, lend themselves less readily than the muscles of others to the invasion of the putrefactive organisms. It is well known that at even comparatively low temperatures fish rapidly putrefy when the atmosphere is loaded with moisture, and that when the atmosphere is dry even at fairly high temperatures putrefaction is comparatively slow, and as dried gelatine is protected from the attacks of most organisms, by drying fish putrefaction is arrested generally in ratio to the completeness of the desiccation.

It is scarcely necessary to point out the practical bearing of this inquiry.

In fish putrefaction, when it once sets in, proceeds much more rapidly than in other vertebrates. This being the case, fish should be used as soon as possible after the rigor disappears. Inspectors of fish markets and fish dealers have various empirical tests by which they believe they are able to determine whether fish are or are not fit for food. They especially trust to the colour of the gills, the firmness and colour of the muscles, and the nature of the odour. As a matter of fact, it is often almost impossible to say whether a fish is or is not fresh after the rigor has disappeared. There is often a pause in the putrefactive process (caused probably by the first crop of bacteria being destroyed by their own bye-products). For this reason it is desirable fish should be used as soon as possible after the rigor has vanished, and that fish, intended for preservation (it matters little how), should be treated, if possible, while the rigor lasts

I have made numerous experiments with ice for preserving fish. It is generally alleged that fish which have been preserved for some time in a frozen state have lost much of their flavour. This I find depends partly on when the freezing is effected and partly on the temperature maintained. Fish which are frozen after the rigor has gone have either very little flavour or they are tainted with offensive septic products. But fish which have been frozen *before* rigor sets in (which have probably never stiffened) are equally without flavour, and they rapidly soften and disintegrate when raised to ordinary temperatures. On the other hand, fish which are frozen immediately after the rigor sets in remain almost unaltered, and when cooked can scarcely be distinguished from fresh fish unless the temperature has been unnecessarily low. The most perfect results were obtained by keeping fish (both salt and fresh water) at a temperature varying from $-1°$ to $-2°$ C. Haddocks which were pithed and gutted and preserved in water-tight insulated chambers at a temperature of $-2°$ C. for three weeks continued rigid from first to last, and when cooked were firmer and better flavoured than ungutted fish only ten hours out of the water. For some reason not easily understood, the fish preserved in water at -1° C. were firmer and better in every way than fish at the same temperature in boxes from which the water escaped as the ice melted. Fish intended for drying and pickling, *i.e.*, for preserving for a long period, should also be treated before or as soon as possible after the rigor goes. When a fish has once begun to disintegrate it is impossible to restore the original freshness, and unless all the flavours are destroyed during the preserving process the results of previous decomposition can easily be detected. Some fish, as curers well know, are incapable of being preserved even with salt, *e.g.*, fish which have died struggling in the water entangled in gill nets are difficult to preserve, because under these conditions, as experiments prove (probably in consequence of the acid reaction thus determined), no distinct rigor ever sets in—death being at once followed by putrefactive changes.

Fish hitherto have usually either been lightly salted and sun-dried, or after being saturated with salt pickled in strong brine. A 20 per cent. salt solution almost completely alters the tissues. Apparently salt owes its preserving power to the fact that it arrests (though it fails to destroy) putrefactive organisms by a process of desiccation—extracting the fluids, without which growth is impossible.

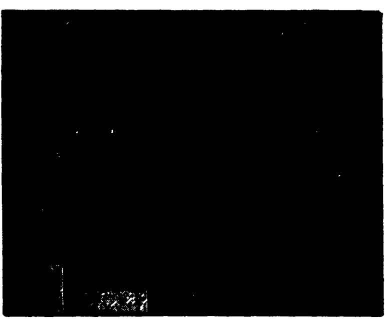
Unfortunately we are acquainted with extremely few substances able in small quantities to arrest the growth of bacteria without rendering the fish unfit for food. It is extremely desirable to at least greatly diminish the amount of salt required This has recently been rendered possible by a process introduced by Mr. Sahlstrom of the Normal Company. In this process the fish are introduced into a cylinder, and, after all the air has been removed by pumping, pressure (5 to 6 atmospheres) is applied to drive the preservative solution (which may contain salt alone, or salt along with other preserving reagents) into the tissues. I have made an extensive series of experiments by this method, and in all cases when fish in a rigid condition were treated, succeeded in arresting putrefactive changes, either permanently or for a limited period according to the strength of the solution used.

This inquiry throws some light on another question which has long been discussed, viz., whether line-caught fish are preferable to fish taken by the beam trawl. In order finally to settle this question, it is only necessary to ascertain whether the rigor disappears quicker in the one case than in the other. I have already mentioned that a line-caught haddock, which has been killed and pithed the moment it leaves the water, may at a temperature of 8° C. remain stiff for 30 hours, *i.e.*, putrefaction may be retarded from 25—30 hours. On the other hand haddocks, captured by a 25-feet beam trawl which had only been two hours at work (large trawls are often down for six hours), even when killed and pithed immediately after taken from the net, may pass into rigor in 30 minutes and be again quite limp 6 hours after death. Sometimes, however, the rigor may not set in for 2 hours after the fish are landed, and it may continue for 17 hours, the difference doubtless resulting partly from the difference in the time the fish were in the trawl net, and partly from the energy expended in attempting to escape, or in endeavouring to maintain the respiratory movements under somewhat difficult circumstances. It may therefore be affirmed that though the rigor may persist as long or nearly as long in some trawled fish as in fish caught with a line, in most cases the rigor disappears sooner from trawled than from line-caught fish; in other words, putrefaction sets in sooner as a rule in fish taken by the trawl than in fish taken by the line, granting, of course, that the line fish are pithed and gutted as soon as they leave the water.

I have, in conclusion, to express my gratitude to Professor Burdon Sanderson and Mr. Gotch for valuable assistance rendered with the experiments made in the Oxford Physiological Laboratory. I am also indebted to Professor Tait for kindly allowing Mr. Lindsay, of the Natural Philosophy Laboratory in the University of Edinburgh, to assist with the electrical experiments. I am further indebted to Mr. Clarkson, B.Sc., of the Natural History Department, Edinburgh, and Mr. W. L. Calderwood and Mr. Jamieson, Members of the Staff of the Fishery Board for Scotland.

XXVI. "Electrochemical Effects on Magnetising Iron." By THOMAS ANDREWS, F.R.S.E., F.C.S. Communicated by Professor G. G. STOKES, P.R.S. Received June 2, 1887.

Having for many years past been engaged in researches relating to the various aspects of the corrosion and oxidation of metals, nearly two years ago it occurred to me to investigate the probable effect of magnetisation on the relative electrochemical position of a pair of bright iron bars, one magnetised by a coil, the other unmagnetised, when thus simultaneously exposed in circuit, in a suitable apparatus, to the action of various powerful oxidising agents and saline solutions. I accordingly specially prepared numerous long polished rods of soft wrought scrap iron 0·261 inch diameter, for use in the investigation. I was not able to commence the preliminary observations until towards the end of 1885, and, after much consideration and various trials then made, decided to adopt the following method of experimentation as perhaps calculated to yield the most delicate and accurate results; pressure of other work has, however, delayed the earlier completion of the work. The general arrangement and methods of experimentation pursued are described below, and the apparatus is delineated on fig 1 and fig 2



Two pieces were adjacently cut from a long finely polished iron rod, so that the pieces might as near as practicable be of identical chemical composition and molecular constitution After being firmly placed and adjusted as to equal length, &c , in the wooden supporting frame W, they were immersed to an exactly equal depth in the solution contained in the U tube, which latter was also rigidly supported by a stand In the duplex experiments made with apparatus fig 2, the U tube was immersed in a large volume, four pints, of cold water to ensure equal temperature conditions during experimentation for the respective solutions in each limb, the cold water being maintained in steady circulation around the tubes The rods were connected in circuit with a sensitive galvanometer, having a resistance of 521 ohms, and of known calibration, the galvanometer being under constant telescopic observation during the experiments, and the normal galvanic action between the two bars previously observed in every experiment Considerable care was requisite to obtain this accurately, so as not practically to interfere with the subsequent results seemingly due to magnetic influences A removable coil C, of stout silk-covered copper wire (No 16 gauge) mounted on a large wooden bobbin 6 inches long enclosed the limb A of the U tube, or when using apparatus fig 2, the coil surrounding the upper portion of the long bar requiring magnetisation. The coil used in the experiments with apparatus fig. 1 consisted of six depths or wraps of the insulated copper wire, each wrap having 81 turns, making in the whole a coil of 486 convolutions. The other coil employed in the experiments with apparatus fig. 2 was of similar construction, but had ten depths of insulated copper wire of the same thickness, constituting a coil with a total of about 750 convolutions.

A single cell bichromate battery, easily put in or out of operation, was attached to the coil, and the battery was recharged with the same strength of solution for each observation. After a suitable time had been allowed in each experiment for steady galvanic equilibrium to be established between the two iron rods, in the solution in the tubes, which took place at periods varying with the nature of the solution, the coil was put into operation. In the experiments with fig. 2 the end of the bar in the solution was the S seeking pole. It was most interesting to observe the result. The rod A thus magnetised in most of the solutions became the metal positive, the galvanometer indicating its steadily increasing electrochemical positive position compared with that of the unmagnetised bar B. Repeated careful experimentation appeared to indicate that the increased positivity of the rod A observed under these conditions was due to the increased action of the acid or saline solution on the iron rod which was under magnetic influence, owing to which it became surrounded by a slightly stronger saline solution than the other unmagnetised rod B, which was apparently less acted upon. In some cases in the more powerfully acid solutions, Table A, columns 4, 5, 7, 8, a kind of maximum point seemed to be generally reached, and after the more violent action of the acid had expended itself, a reduction of the E.M.F. between the rods was generally noticed as the solution in the B-tube gradually approached an equilibrium of composition compared with the solution in the A limb of the U-tube, and subsequently a reverse action in some cases was observed. The unmagnetised rod B appeared to be less rapidly acted upon than the one under magnetic influence. On magnetisation of the bar the above full effect on the galvanometer was not always of an instantaneous character, though a short time only appeared requisite for its development The solutions employed are given in Table A, and the results therein recorded were derived from a series of constant observations, a comparison in some instances being afforded between the respective effects obtained by the two forms of apparatus employed.

It may be noticed that a fresh pair of the iron rods, cut adjacently from a long polished rod, were used for each experiment, and 123 pairs were used in course of this part of the investigation, the whole of the experiments being many times repeated to ensure accuracy, the results recorded being the average of many observations. In some instances somewhat higher results were noticed.

It is almost impossible to obtain two pieces of iron (even when forming adjacent parts of one polished rod) which when in solution are devoid of some slight galvanic action between themselves; but the greatest care was exercised in the special preparation of the iron used, so that this variation might be reduced to a minimum.

To ensure success in the experiments it was found essential that the iron bars should possess an excellent polished surface, free from magnetic or other oxide or impurities; the solutions were also concentrated, and both discrimination and manipulative skill were requisite in obtaining the practical galvanic equilibrium of the bars at commencement. The time needed to ensure this seemed to vary considerably with different solutions according to circumstances. A sensitive galvanometer was also a requisite of success in these observations, and telescopic readings were necessary, as in some cases the effects were small

It seems desirable here to add a few remarks on the possible influence of temperature on the reactions, and to state the means used in the endeavour to minimise errors from this source. In conducting the experiments, I should have preferred using greater battery power, but employed only *one* bichromate cell; the wire of the coil was also of considerable thickness to prevent undue heating from resistance. The centre of the wooden coil bobbin was also about inch in thickness, so as to act as a central non-conductor. Moreover, an air space was allowed of $\frac{1}{8}$ inch between the wooden centre of the coil and the enclosed limb of the U-tube. The other limb for the unmagnetised bar was enclosed by another coil, which, when not in use, acted as an external protective jacket. Notwithstanding these precautions, there was a slight increase of temperature in the interior of the coil C. Thermometers inserted in test solutions, one in each limb, gave an average difference of about 1° Fahrenheit at the end of an hour, this increase of temperature in the solution in the coil tube being, however, very gradual. It would be untenable to state that this difference of temperature, arising from the action of the coil, did not to some slight extent influence the results of the experiments with apparatus fig. 1; but the results obtained therewith could certainly not be regarded as due only to differences of temperature conditions between the two tubes. Most of the experiments with that apparatus afford within themselves evident proof to the contrary; thus, it will be seen, that the magnetised bar assumed a prominent positive position almost immediately after magnetisation, in the case of nitric acid, before any perceptible difference of temperature could obtain between the respective tubes (see Table A, columns 6 and 7).

Moreover, the exceptional negative position of the magnetised bar under the same temperature conditions, in the case of sulphuric acid, affords evidence that the effect was not due to these temperature causes To more clearly demonstrate however, that the results were mainly due to the influence of magnetisation, and acting on the kind suggestion of Professor G G Stokes it was decided to make a duplex series of observations I accordingly devised the modified form of apparatus fig 2, which was intended to eliminate possible sources of error from temperature difference, by keeping the **U** tube surrounded by a large volume of cold water during the experiments the solution in the respective tubes being thus maintained under equal conditions In comparing the results obtained with the two forms of apparatus, it should be borne in mind that when using the apparatus fig 2 the cold water surrounding the **U** tube would have a tendency to retard the increase of the temperature of the solutions naturally arising from chemical combination Further the coil in fig 2 being at one end of the long bar would be calculated to modify the magnetisation of the other end of the metal in the solution, compared with its action in fig 1, where the coil almost entirely surrounded the bar hence in fig 2, the coil was made somewhat larger to overcome this to some extent, and the end of the bar B was shortened in experiments with fig 2, so that this bar would be less liable to be affected magnetically by the external influence of the larger coil I hope that the confirmatory results obtained in the two sets of observations may be considered as fairly satisfactory

Table A.

Time from commencement and duration of magnetisation in minutes.		E.M.F. in volt, and electrochemical position of magnetised bar compared with the unmagnetised bar, the positive or negative position of the former being respectively indicated by the signs + and −.					
		Column 1.		Column 2.		Column 3.	
		Potassium chlorate and one-fifth nitric acid.		Potassium chlorate and one-third nitric acid		Potassium chlorate and hydrochloric acid	
		I.	II.	I.	II.	I.	II.
hrs	mins.						
0	0	0·000	0·000	0·000	0·000	0·000	0·000
0	1	..	+0·004	+0·014	+0·002	..	..
0	2	..	..	..	+0·003	..	..
0	2½	..	+0·006	+0·014	+0·004	..	+0·002
0	3	..	..	..	+0·005	..	..
0	4	..	..	+0·016	+0·006	..	..
0	5	+0·005	+0·006	+0·011	+0·007	+0·002	+0·008
0	6	..	..	..	+0·007	..	..
0	7	..	..	..	..	..	..
0	7½	..	+0·004	+0·011	+0·008	..	+0·006
0	8	..	..	..	..	..	..
0	9	..	..	..	..	..	..
0	10	+0·006	+0·004	+0·011	+0·008	+0·004	+0·004
0	11	..	..	..	..	..	..
0	12	..	..	..	..	..	..
0	12½	..	..	..	..	..	..
0	13	..	..	..	..	..	..
0	14	..	..	..	..	..	..
0	15	+0·005	+0·008	+0·010	+0·007	+0·003	+0·004
0	17½	..	..	..	+0·007	..	..
0	20	+0·006	+0·008	+0·010	+0·007	+0·007	+0·004
0	25	+0·006	+0·003	+0·010	+0·007	+0·008	+0·004
0	30	+0·007	+0·002	+0·010	+0·009	+0·007	+0·005
0	35	+0·007	+0·003	+0·011	+0·006	+0·009	+0·004
0	40	+0·007	+0·008	+0·011	+0·006	+0·008	+0·006
0	45	+0·008	+0·008	+0·012	+0·005	+0·007	+0·005
0	50	+0·009	+0·002	+0·011	+0·005	+0·006	+0·005
0	55	+0·008	+0·008	+0·012	+0·005	+0·007	+0·006
1	0	+0·007	+0·003	+0·013	+0·005	+0·007	+0·005
1	5	..	+0·005	..	+0·004	..	+0·009
1	15	..	..	..	+0·005	..	+0·008
1	30	..	+0·007	..	+0·006	+0·009	..
1	45	..	+0·006	..	..	+0·009	..
2	0	..	+0·005	..	+0·005	+0·011	..

Column 1.—The potassium chlorate was a saturated solution of the salt, to which was added one-fifth of its volume of nitric acid of sp. gr. 1·388 at 60° F.

Column 2.—The potassium chlorate was a saturated solution of the salt, to which was added one-half of its volume of nitric acid of sp. gr. 1·388.

Column 3.—The potassium chlorate was a saturated solution, to which was added an equal volume of hydrochloric acid of sp. gr. 1·16.

Column 4.—The potassium bi-chromate was a saturated solution, to which was added one-half of its volume of nitric acid of sp. gr. 1·388.

Column 5.—The ferric chloride was a saturated solution, to which was added one-half of its volume of nitric acid of sp. gr. 1·388.

Table A—*continued.*

Time from commencement and duration of magnetisation in minutes.		E.M.F. in volt, and electrochemical position of magnetised bar compared with the unmagnetised bar, the positive or negative position of the former being respectively indicated by the signs + and −.				
		Column 4.	Column 5.		Column 6.	
		Potassium bi-chromate and nitric acid.	Ferric chloride and nitric acid.		Nitric acid, sp. gr. 1·388, one part, and three parts water.	
		II.	I.	II.	I.	II.
hrs.	mins.					
0	0	0·000	0·000	0·000	0·000	0·000
0	1	..	..	−0·014	+0·026	+0·004
0	2	+0·009	..	+0·002	..	..
0	2½	+0·005	..	+0·002	+0·020	+0·005
0	3	+0·009	..	+0·011	+0·020	+0·007
0	4	+0·010	..	+0·013	+0·016	+0·009
0	5	+0·007	..	+0·013	+0·014	+0·009
0	6	..	..	+0·020	..	..
0	7	..	..	+0·016	..	..
0	7½	..	..	+0·006	+0·011	+0·011
0	8	..	..	+0·022	..	..
0	9	.	..	+0·034	..	+0·011
0	10	+0·006	..	+0·011	+0·008	+0·011
0	11	..	..	+0·014	..	..
0	12	..	..	+0·011	..	..
0	12½	+0·009	..	..	..	+0·010
0	13	..	..	+0·009	..	..
0	14	..	..	..	..	..
0	15	+0·004	..	+0·002	+0·009	+0·005
0	16	..	..	..	..	..
0	17	..	..	..	..	..
0	17½	..	..	+0·009	..	..
0	19	..	..	..	..	..
0	20	+0·007	+0·009	+0·007	+0·007	+0·005
0	22½	..	..	..	..	..
0	25	+0·005	+0·018	+0·004	+0·001	+0·003
0	30	+0·003	+0·038	+0·002	+0·001	+0·004
0	35	+0·001	+0·038	+0·003	+0·011	+0·004
0	40	+0·004	+0·023	+0·002	+0·012	+0·004
0	45	+0·001	+0·002	..	+0·013	+0·004
0	50	+0·002	+0·004	..	+0·009	+0·006
0	55	..	+0·005	..	+0·012	+0·007
1	0	+0·006	+0·006	+0·001	+0·013	+0·007
1	5	..	..	..	..	..
1	15	..	..	..	..	..
1	30	..	..	..	..	..
1	45	..	..	..	..	..
2	0	..	+0·007	..	..	..
2	20	..	..	..	..	..
3	0	..	..	..	..	..
3	15	..	..	..	..	..

Table A—*continued.*

Time from commencement and duration of magnetisation in minutes.		E M F. in volt, and electrochemical position of magnetised bar compared with the unmagnetised bar, the positive or negative position of the former being respectively indicated by the signs + and −.					
		Column 7.		Column 8	Column 9	Column 10.	
		Nitric Acid, sp gr 1·388, 1 part, and 4 parts water.		Aqua regia, 2 parts HNO_3 and 1 part HCl, diluted to one-half with water	Hydro-chloric acid, sp gr. 1 16, diluted to one-half with water.	Sulphuric acid (concentrated), sp. gr. 1·84.	
		I	II.	II.	II.	I	II.
hrs.	mins.						
0	0	0·000	0·000	0·000	0·000	0·000	0·000
0	1	..	+0·004	+0·002	..	..	..
0	2½	..	+0·006	..	..	..	−0·001
0	3	..	+0·009	..	..	..	..
0	4	..	+0·012	..	..	..	..
0	5	+0·012	+0·011	+0·004	−0·0004	−0·003	−0·002
0	6	..	+0·010	..	..	..	..
0	7½	..	+0·007	+0·006	..	..	..
0	9	..	..	..	..	..	..
0	10	+0·011	+0·003	+0·011	−0·001	−0·004	−0·003
0	11	..	+0·005	..	..	..	..
0	12	..	+0·005	+0·013	..	..	..
0	12½	..	..	..	..	..	..
0	13	..	+0·005	..	..	..	..
0	14	..	+0·003	+0·014	..	..	..
0	15	+0·013	+0·001	+0·027	−0·001	−0·005	−0·005
0	16	..	..	+0·027	..	..	..
0	17	..	..	+0·014	..	..	..
0	19	..	..	..	..	..	..
0	20	+0·014	+0·004	+0·025	−0·001	−0·006	−0·005
0	22½	..	..	+0·010	−0·001	..	..
0	25	+0·013	+0·004	..	−0·001	−0·005	−0·006
0	30	+0·012	+0·005	+0·004	−0·001	−0·007	−0·006
0	35	+0·013	+0·005	..	−0·001	−0·008	−0·006
0	40	+0·012	+0·004	..	−0·001	−0·009	−0·006
0	45	+0·012	+0·003	+0·001	−0·001	−0·011	−0·005
0	50	+0·012	+0·003	..	−0·001	−0·012	−0·005
0	55	+0·012	+0·004	+0·001	−0·002	−0·014	−0·006
1	0	+0·011	+0·004	+0·001	−0·002	−0·016	−0·006
1	15	..	..	..	−0·003	−0·022	−0·007
1	30	..	..	..	−0·002	−0·022	−0·008
1	45	..	..	..	−0·004	..	−0·008
2	0	..	..	..	−0·004	..	−0·009
2	20	..	..	..	..	..	−0·028
3	0	..	..	..	..	..	−0·009
3	15	..	..	..	..	..	−0·021

The records in the above table under divisions I were experiments made with apparatus, fig. 1, the records entered under divisions II, relate to observations made with apparatus, fig. 2.

The records of the effects in the stronger solutions do not indicate the full extent of the electric action compared with that of the weaker solutions; because in the former case the tubes could only be partially filled, so as to prevent boiling over.

General Remarks.

Potassium Chlorate and Nitric Acid Solutions, Columns 1 *and* 2.—At the termination of some of these experiments, the depth of colour of the solution surrounding the magnetised iron was perceptibly of a darker shade than the colour in the tube surrounding the unmagnetised bar, this was confirmed by Eggertz's carbon coloration test.

Ferric Chloride and Nitric Acid, Column 5.—At forty minutes from the commencement of one experiment in apparatus fig. 1, the record was an E.M.F. of 0·023 volt, the magnetised bar being positive; the battery was then attached to a smaller coil surrounding the other bar B, which was then magnetised instead of the bar A, and in course of five minutes a reduction in the positive position of bar A to an extent of 0·021 volt occurred. At forty-five minutes the battery was reconnected to the coil surrounding the bar A, producing a steady increase of positive position in that bar as recorded in Table A, column 5.

Aqua Regia, undiluted.—With this reagent in fig 1 no very decided galvanic reaction took place until the bar had remained magnetised for some ten minutes, when violent effervescence occurred accompanied by evolution of dense reddish-brown fumes, the magnetised bar then becoming rapidly electropositive to an extent yielding an E.M.F. of about 0·110 volt. This position was subsequently more or less maintained for some ten minutes, the galvanometer, however, gradually falling to zero as the ebullition in both tubes subsided. This was a difficult experiment to make, owing to the very violent effervescence.

Aqua Regia, diluted, Column 8.—Up to the commencement of the effervescence no perceptible difference in the colour of the solution in the respective limbs of the U-tube was noticed; but immediately on the violent ebullition occurring, which took place generally about ten minutes from commencement, the solution surrounding the magnetised bar frequently became of a very much darker tint. This marked difference between the colour of the respective solutions in the two tubes was maintained for some time, afterwards the two solutions became apparently nearly chromatically equal. The electrochemical effect appeared to be less marked when using aqua regia containing excess of HCl. The relative electrochemical position of the two bars in such case being less divergent, the HCl appearing to act somewhat as a strong diluent.

Hydrochloric Acid and Sulphuric Acid, Columns 9 *and* 10.—Singular to say, the previously described magnetochemical effects were comparatively small when using such a powerful reagent as hydrochloric acid alone, either concentrated (sp. gr. 1 16) or diluted. When this acid was employed in conjunction with concentrated solution of potassium chlorate (Table A, column 3), the effects there recorded appeared due rather to the oxidising agency of the evolved chloric compounds; in the presence of magnetism, the reactions with this electrolyte were occasionally irregular, the excess of HCl appearing sometimes to interfere Sulphuric acid, conc., also seemed to behave abnormally, though this acid does not ordinarily act strongly on iron. The experiments with both the above acids in course of a large number of observations, proved exceptions to the general rule. The magnetised bar in the sulphuric acid, conc., and also in the hydrochloric acid, diluted, became the electronegative metal, not only in the case of H_2SO_4, when using the apparatus fig. 1, but also when experimenting with the modified form, fig. 2. Occasionally, with sulphuric acid, conc., the magnetised bar was, on first magnetising it, slightly electropositive for a few minutes only; but afterwards became steadily negative.

Under the conditions of experimentation, the magnetised bars in the powerful oxidising reagents used almost invariably assumed the electropositive position, the presence of $H.NO_3$ appearing essential to the full development of the positive position of the bar under the influence of magnetism. On the contrary, the magnetised bar seemed to be the electronegative metal in H_2SO_4, conc., and also in the HCl (diluted), as electrolytes. The two latter reagents by their action on the metal generate gases of a reducing character. These exceptions are not, however, averse to the principle that magnetisation exerts an influence on the relative electrochemical position of a pair of iron bars, varying according to the nature of the solution and the extent to which one of them is magnetised. In the above exceptions, it is possible that the magnetised bar assumed the negative position consequent on its being the one more attacked, under magnetic influences, by the reagent; thus producing a greater evolution of reducing gases in the tube A containing the magnetised bar than in the other tube; this may perhaps explain the negative effect. The observations of this memoir therefore indicate that, under the powerful and rapidly oxidising conditions described, a magnetised bar becomes metal positive to an unmagnetised one, whereas in the exceptional instances above alluded to, the electronegative effect occurs, possibly owing to the presence in the solution of such reducing agents as nascent hydrogen, &c.

In the present incomplete stage of the enquiry these remarks are only offered tentatively.

The effects could not be expected to be large; I anticipate, however, generally more marked results in a more powerful magnetic field, exerting its influence, perhaps, for longer periods; but I think the experiments now submitted appear sufficient at least to afford an indication that, under the conditions recorded, magnetisation exerts on iron, in some solutions, an appreciable effect. The results already obtained in this direction are so far interesting as to encourage further research into the nature of this novel and subtle phenomenon.

XXVII. "Note on the Functions of the Sinuses of Valsalva and Auricular Appendices, with some Remarks on the Mechanism of the Heart and Pulse." By M. COLLIER. Communicated by VICTOR HORSLEY, F.R.S., Professor Superintendent of the Brown Institution. Received June 9, 1887.

(Abstract.)

The object of the paper is to disprove the present apparently accepted idea, that the sinuses of Valsalva are mere bulgings of the arterial walls, formed by a reflex current induced by the sudden closure of the semilunar valves.

The existence of a reflex current is shown to be impossible, and the theory of the sudden opening and closure of the semilunar valves is strongly opposed.

The presence of the sinuses of Valsalva is urged as an absolute essential to the mechanism of the heart's action. The paper then treats of the action of the auricle and the part played by the auricular appendix, the latter being considered as the only part of the auricle that sensibly and vigorously contracts.

The causes of the first sound of the heart are next alluded to, and the theory that the closure and vibration of the tricuspid and mitral valves assist in its production is refuted. The action of the ventricle and the mode of the injection of its contents into the aorta is dwelt upon at some length.

The latter part of the paper is devoted to the mechanism of the pulse, and an explanation is given of the so-called dicrotism.

The paper terminates with a summary of the chief points of the conclusions arrived at.

XXVIII. "On Hamilton's Numbers." By J. J. SYLVESTER, F.R.S., Savilian Professor of Geometry in the University of Oxford, and JAMES HAMMOND, M.A. Cant. Received June 11, 1887.

(Abstract)

In the year 1786 Erland Samuel Bring, Professor at the University of Lund in Sweden, discovered that by the method of Tschirnhausen it was possible to deprive the general algebraical equation of the 5th degree of three of its terms without solving an equation higher than the 3rd degree. By a well understood, however singular, academical fiction, this discovery was imputed by him to one of his own pupils, one Sven Gustaf Sommelius, and embodied in a thesis humbly submitted to himself for approval by that pupil, as a preliminary to his obtaining his degree of Doctor of Philosophy in the University.* It seems to have been overlooked or forgotten, and was subsequently re-discovered many years later by Mr. Jerrard. In a report contained in the 'Proceedings of the British Association' for 1836, Sir William Hamilton showed that Mr. Jerrard was mistaken in supposing that the method was adequate to taking away more than three terms of the equation of the 5th degree, but supplemented this somewhat unnecessary refutation by a profound and original discussion of a question raised by Mr. Jerrard, as to the number of variables required in order that any system of equations of given degrees in those variables shall admit of being satisfied without solving any equation of a degree higher than the highest of the given degrees.

In the year 1886 the senior author of this memoir showed in a paper in Kronecker's (better known as Crelle's) 'Journal' that the trinomial equation of the 5th degree, upon which by Bring's method the general equation of that degree can be made to depend, has necessarily imaginary coefficients except in the case where four of the roots of the original equation are imaginary, and also pointed out a method of obtaining the absolute minimum degree M of an equation from which any given number of specified terms can be taken away subject to the condition of not having to solve any equation of a

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* Bring's Reduction of the Quintic Equation was republished by Mr. Robert Harley, F.R.S., in the 'Quarterly Journal of Pure and Applied Mathematics,' vol. 6, 1864, p. 45. The full title of the Lund Thesis, as given by Mr. Harley (see 'Quart. Journ Math.,' pp. 44, 45) is as follows: "B. cum D. Meletemata quaedam mathematica circa transformationem aequationum algebraicarum, quae consent. Amplis. Facult. Philos in Regia Academia Carolina Praeside D. Erland Sam Bring, Hist. Profess. Reg. & Ord. publico Eruditorum Examini modeste subjicit Sven Gustaf Sommelius, Stipendiarius Regius & Palmcrentzianus Lundensis. Die XIV Decemb, MDCCLXXXVI, L.H Q.S.—Lundae, typis Berlingianis."

degree higher than M. The numbers furnished by Hamilton's method, it is to be observed, are not *minima* unless a more stringent condition than this is substituted, viz., that the system of equations which have to be resolved in order to take away the proposed terms shall be the simplest possible, *i.e.*, of the lowest possible weight and not merely of the lowest order; in the memoir in 'Crelle' above referred to, he has explained in what sense the words weight and order are here employed. He has given the name of Hamilton's Numbers to these relative minima (minima, *i.e.*, in regard to weight), for the case where the terms to be taken away from the equation occupy consecutive places in it, beginning with the second.

Mr. James Hammond has quite recently discovered by the method of generating functions a very simple formula of reduction, or scale of relation, whereby any one of these numbers may be expressed in terms of those that precede it: his investigation, which constitutes its most valuable portion, will be found in the second section of this paper. The principal results obtained by its senior author consequential in great measure to Mr. Hammond's remarkable and unexpected discovery, refer to the proof of a theorem left undemonstrated in the memoir in 'Crelle' above referred to, and the establishment of certain other asymptotic laws to which Hamilton's Numbers and their differences are subject, by a mixed kind of reasoning, in the main apodictic, but in part also founded on observation. It thus became necessary to calculate out the 10th Hamiltonian Number, which contains 43 places of figures. The highest number calculated by Hamilton (the 6th) was the number 923, which comes third in order after 5 (the Bring number), 11 and 47 being the two intervening numbers. It is to be hoped that some one will be found willing to undertake the labour (considerable but not overwhelming) of calculating some further numbers in the scale, in order to establish or disprove conclusively the presumptive law of the asymptotic branch of the series connecting any two consecutive semi-differences η_s, η_{s+1} of the Hamiltonian Numbers, viz.:—

$$\eta_{s+1}-\eta_s^2=\eta_s^{\frac{3}{2}}\Sigma_{r=0}^{r=\infty}c_r\,\eta_s^{(\frac{1}{2})^r}$$

The theory has been "a plant of slow growth" The Lund Thesis of December, 1786 (a matter of a couple of pages), Hamilton's Report of 1836, with the tract of Mr. Jerrard therein referred to, and the memoir in 'Crelle' of December, 1886, constitute as far as the senior author of this paper is aware, the complete bibliography of the subject up to the present date.

XXIX. "On the Induction of the Explosive Wave and an Altered Gaseous Condition in an Explosive Gaseous Mixture by a Vibratory Movement." By LEWIS T. WRIGHT. Communicated by Professor ODLING, F.R.S. Received June 13, 1887.

(Abstract.)

The author refers to the conclusions of Berthelot and Vieille that the phenomenon of the explosive wave is quite distinct from that of ordinary combustion, each being marked by well-defined limits called by them the *régime* of detonation and combustion respectively. The transition from the one to the other is accompanied by violent vibratory movements.

Mallard and Le Chatelier separate the combustion of an explosive mixture inflamed at the open end of a tube, closed at the other, into four different and succeeding phases—

(1.) Uniform propagation of flame;
(2.) A vibratory movement; followed in some cases by
(3.) The explosive wave of Berthelot and Vieille.
(4.) Spontaneous extinction of flame.

The author has specially studied the connexion between the vibrating stage and the explosive wave with a certain mixture of coal gas and air (in large glass tubes) which sharply exhibits the various features of the four stages described by Mallard and Le Chatelier.

The points determined were these, that the detonating stage (explosive wave) is never initiated without preceding vibratory movements on the part of the flames.

That with the same mixture the vibrating period is of definite duration culminating in the explosive wave stage.

The necessary connexion between the two stages being proved, the author investigated the question whether the explosive wave condition is communicated layer by layer by the contact of the flame itself, or whether the whole column of unignited gas in the tube adjacent or distant from the flame is "induced" by the vibrating flame into a more receptive condition which enables the chemical reaction between the molecules to proceed at a more rapid rate than usual.

The phenomena exhibited by the flame suggest this latter explanation, and the author by the application of a weak spark test has been enabled to prove that the whole column of gas, either adjacent or distant from the vibrating flame, is in an altered condition after being submitted to but a portion of the vibratory action which normally initiates the explosive wave.

An electric spark of low tension not capable of igniting the uninduced explosive mixture invariably does so, after the vibrating has been set up.

It is suggested that the tremor sent through the unignited gas synchronised some of the molecular vibrations, so that the molecules capable of reacting perform their translatory movements in some measure together, and that when a focus of inflammation is present more reacting molecules come into the sphere of inflammation in a given time, and therefore the rate of inflammation is more rapid.

XXX. "Note on Communication entitled 'Preliminary Note on a Balanoglossus Larva from the Bahamas' ('Roy. Soc. Proc.,' vol. 42, p. 146)." By W. F. R. WELDON, M.A. Communicated by Professor M. FOSTER, Sec.R.S. Received June 16, 1887.

In a paper, communicated to the Royal Society in March last, I described a series of Balanoglossus larvæ, found by me in the Bahama Islands. The series extended from a larva with one pair of gill-slits to a form resembling in many ways a normal Tornaria; but the differences between this larva and the normal European form were so great as to induce me to believe that a process of degeneration was going on, and that the Tornaria-like creature was the oldest, not the youngest, of the series.

On seeing my paper, Professor Spengel, whose researches on Balanoglossus are well known, wrote to me, informing me that I was altogether mistaken in my interpretation of the larvæ which I had found, and that my series belonged in fact to the normal order of development.

By the courtesy of Dr. Spengel I have been enabled to inspect his magnificent series of preparations, illustrating the whole life-history of Balanoglossus, and so to become convinced of the truth of his statement; I now, therefore, take the earliest opportunity of withdrawing my previous statement, and desire to express my regret at having placed such an erroneous doctrine on record in the 'Proceedings' of the Society.

I beg also to thank Dr. Spengel most sincerely for his kindness to me in this matter.

XXXI. "Note on the Anatomy of Asiatic Cholera as exemplified in Cases occurring in Italy in 1886." By CHARLES S. SHERRINGTON, M.B., M.A. Communicated by Professor M. FOSTER, Sec. R.S. Received June 16, 1887.

Last summer when cholera again appeared in Italy I determined to seize the opportunity that seemed to offer itself for re-examining the disease, especially with regard to some questions raised by the work of the previous year.

Although, on grounds which I have already detailed in a letter to the Secretary of the Society, Professor Michael Foster, the conditions imposed by the state of the country quite precluded the carrying out there and then of trustworthy experiments with the living contents of the dejecta and of the viscera of the cholera patients, still it was possible to collect a satisfactory amount of anatomical material. The results in this direction of an expedition, a part of the expenses of which were generously borne by the Society, were promised to the Society in the letter above alluded to. The investigation of the material has now been completed, and may briefly be stated as follows:—

The material collected consists in all of specimens and preparations made from twenty-five fatal cases. Of these cases twenty-two were indubitable examples of rapidly fatal cholera asiatica. These were obtained exclusively out of the Province of Puglia. The remaining three cases were from Venetia, and were in the opinion of myself and Dr. Rouse, who in Venetia assisted me, not examples of true cholera. At each autopsy the specimens taken for preservation and subsequent detailed microscopical investigation were portions of the stomach and intestinal canal at various points, of the thoracic and abdominal organs, and of the mesenteric glands. From the contents of the stomach and intestine preparations for the microscope were made at the same time by the Ehrlich-Koch method. From the vomit and dejecta of six moribund patients cover-glass preparations were also made in the same manner. With regard to the hardening of the tissues for microscopical examination, the portions of the tissue preserved were at the autopsy carefully placed separately into specimen glasses containing absolute alcohol, and the alcohol was at the end of six hours renewed, the specimen being at the same time quickly cut into pieces never more than 3 mm. thick. The alcohol was then changed at the end of twelve hours, and again at the end of twenty-four, then not again for a week.

The microscopical investigation has been carried on in the laboratory of Professor Virchow at Berlin; in his debt I stand for much kindness and liberality.

One of the main objects of the renewed inquiry was to ascertain the presence in, or the absence from, the anatomy of cases of cholera from another epidemic than the Spanish of certain appearances that the microscopical preparations from the Spanish epidemic of 1885 had presented. The appearances referred to were described in a Report made to the Society in June of last year, and printed in the Proceedings of the Society for that year, so that it is unnecessary for me to repeat them here. The more so since in this year's work I have completely failed after minute, long and repeated search, with the use of good lenses (new apochromatic system of Zeiss), and after employment of various methods of staining including that by which the Spanish preparations were coloured, to find any trace of the above-mentioned appearances in any of the material obtained in Italy. Neither in the specimens of the tissues nor of the intestinal fluid post mortem, nor of the vomit or dejecta during life is any trace of them to be found. Any view that suggested itself of a causal connexion of them with cholera must therefore meet the difficulty that they form no constant anatomical feature of the disease.

With regard to the presence of comma-shaped bacilli in my material, such forms have been found in altogether thirteen of the cases from Puglia, although always with difficulty, and seven times only after extremely patient and rigorous search. The method found most satisfactory for their detection has been that of Loffler with a methylene-blue solution made according to the receipt given by him. The chief difficulties of the investigation have lain in the facts, that the comma-bacilli are among the bacilli which are earliest decolorised by the solutions for removing the excess of stain, that the morphological characters of the comma-bacilli are not so distinctive as to make their recognition from bacilli of some other species always certain, and that in none of my specimens have I found them free from admixture with other micro-organisms, and in none in very great abundance. Having only the morphological characters of the bacilli for criterion, I have compared them always with specimens from pure cultivations of Koch's comma-bacillus freshly prepared for the purpose, and I have only accepted them as such when they have agreed with the latter standard form.

Of the cases in which the comma-bacilli have been found, in three they may be called "fairly numerous," in five "sparse," and in five "very scanty." The bacilli have never been found in any other situation than in the wall of the alimentary canal, and in the wall only in the most superficial portion of the tissue, in the mucosa.

The three cases in which they are fairly numerous are characterised clinically by the fatal ending having supervened without any stage of febrile reaction, and anatomically by the changes in the wall of the intestine being confined to partial denudation from epithelium of the villi, especially, to the presence of an excessive number of leucocytes in the meshes of the mucosa and submucosa, and to evident engorgement of the portal venules. The comma-shaped bacilli lie in the fundi of the tubular glands of, especially, the ileum, and in the tissue in which those glands are imbedded in the immediate vicinity of the glands. Their distribution is not uniform, but is patchy. They occur with various other forms of bacteria in the same situation. Generally of these other forms some have penetrated more deeply into the tissue than have the comma-bacilli; especially is this true of certain fine, straight bacilli resembling morphologically the bacterium coli commune of Escherich.

In the other cases in which comma-bacilli are found, the signs of acute severe inflammation are more obvious, many of the denuded villi are in part necrotic. Shallow sloughs occupy the surface of the mucous membrane, together with small extravasations and patches of "coagulation-necrosis." Here the comma-shaped bacilli are within the mucosa, and with them occur a multitude of micrococci and bacilli of various shape and size Although in two of these cases bacilli are to be seen in the distended venules, I have in no instance found comma-bacilli within any blood-vessel.

Of those cases in which comma-bacilli have not been found, the impression left upon me is that in some of them it is possible that still further examination of a still more extended series of preparations from them might have revealed comma-bacilli in the wall of the intestine in some of them. They are all cases that, although of rapidly fatal issue, had passed into a stage of febrile reaction.

In them often the surface of the ileum and large intestine was thickly set with little patches of superficial sloughing, and microscopical preparations made through these areas forcibly recall preparations of a mucous membrane diphtheritically inflamed. The attached face of the slough is occupied frequently by an almost continuous sheet of bacteria, and bacteria infiltrate the inflamed submucosa, and often the superficial regions of the muscularis. In three of these cases micro-organisms are present in the blood-vessels, more especially numerously in the venules of the portal system.

With regard to the preparations made from freshly evacuated dejecta and vomit from living cases, the six cases examined reveal comma-bacilli in the stools in five. There is no evidence of blood in these five stools, but blood is mixed with the intestinal fluid in the case in which no comma-bacilli are seen. The vomit also in three of the cases shows a small number of comma-bacilli. In none of the preparations do the comma-bacilli make up more than a small fraction of all the bacterial forms present.

With regard to the statement by Cohnheim that the shedding of the intestinal epithelium is purely and merely a process setting in post mortem, this, which has always been denied by Virchow and others, is negatived by the occurrence in the stools here examined of an abundance of epithelial cells, often very slightly differing in appearance from the normal. Occasionally they are coherent as groups of four and five; there are, however, no finger-shaped casts of complete villi.

The cases of a doubtful nature from Venetia have not disclosed any comma-bacilli under microscopical examination. In one of them the ulcers present in the ileum, which to the naked eye resembled those of enteric fever, pass deeply into the thickness of the muscular coat of the intestine, a condition to which I have only once seen any close approach in Asiatic cholera.

XXXII. "On certain Definite Integrals. No. 15." By W. H. L. RUSSELL, F.R.S. Received June 16, 1887.

Mr. Fox Talbot's researches on the comparison of transcendents are well known. The following are founded on the same principle, applied in a different manner:—

Let—

$$a_1x^3+b_1y^3+c_1z^3 = e_1,$$

$$a_2x^3+b_2y^3+c_2z^3 = e_2,$$

be two equations connecting the variables x, y, and z. Then we can find x and y in terms of z, x and z in terms of y, y and z in terms of (x). Or if—

$$b_1c_2-c_1b_2 = \mathrm{A}_1, \quad c_1a_2-a_1c_2 = \mathrm{B}_1, \quad a_1b_2-b_1a_2 = \mathrm{C}_1;$$

and also

$$e_1a_2-e_2a_1 = \mathrm{A}_2, \quad e_1b_2-e_2b_1 = \mathrm{B}_2, \quad e_1c_2-e_2c_1 = \mathrm{C}_2;$$

we shall have

$$x = \frac{1}{\sqrt[3]{(\mathrm{C}_1)}}\sqrt[3]{(\mathrm{B}_2+\mathrm{A}_1z^3)},$$

$$y = -\frac{1}{\sqrt[3]{(\mathrm{C}_1)}}\sqrt[3]{(\mathrm{A}_2-\mathrm{B}_1z^3)}$$

$$z = \frac{1}{\sqrt[3]{(\mathrm{B}_1)}}\sqrt[3]{(\mathrm{A}_2+\mathrm{C}_1y^3)},$$

$$x = -\frac{1}{\sqrt[3]{(B_1)}}\sqrt[3]{(C_2-A_1y^3)},$$

$$y = \frac{1}{\sqrt[3]{(A_1)}}\sqrt[3]{(C_2+B_2x^3)},$$

$$z = -\frac{1}{\sqrt[3]{(A_1)}}\sqrt[3]{(B_2-C_1x^3)}.$$

Now, since

$$\int dx\,.\,zy+\int dy\,xz+\int dz\,xy = xyz,$$

$$(B_1C_1)^{1}\int_{\lambda_2}^{\lambda_1} dx\sqrt[3]{(C_2+B_1x^3)}\sqrt[3]{(B_2-C_1x^3)}$$

$$+(A_1C_1)^{1}\int_{\mu_2}^{\mu_1} dx\sqrt[3]{(A_2+C_1x^3)}\sqrt[3]{(C_2-Ax^3)}$$

$$+(A_1B_1)^{1}\int_{\nu_2}^{\nu_1} dx\sqrt[3]{(B_2+A_1x^3)}\sqrt[3]{(A_2-B_1x^3)}$$

$$= (A_1B_1C_1)^{1}(\lambda_2\mu_2\nu_2-\lambda_1\mu_1\nu_1),$$

where the limits $\lambda_1\lambda_2\mu_1\mu_2\nu_1\nu_2$ must satisfy the equations—

$$a_1\lambda_1^3+b_1\mu_1^3+c_1\nu_1^3 = e_1,$$

$$a_1\lambda_2^3+b_1\mu_2^3+c_2\nu_2^3 = e_2,$$

$$a_2\lambda_1^3+b_2\mu_1^3+c_2\nu_2^3 = e_2,$$

$$a_2\lambda_2^3+b_2\mu_2^3+c_2\nu_2^3 = e_2;$$

from which it appears that one pair of limits may be considered arbitrary, while the other two pairs are given by these equations.

Since—

$$\int\frac{dx}{x^2yz}+\int\frac{dy}{xy^2z}+\int\frac{dz}{xyz^2} = -\frac{1}{xyz},$$

$$A_1^{1}\int_{\lambda_2}^{\lambda_1}\frac{dx}{x^2\sqrt[3]{(C_2+B_1x^3)}\sqrt[3]{(B_2-C_1x^3)}}$$

$$+B_1^{1}\int_{\mu_2}^{\mu_1}\frac{dx}{x^2\sqrt[3]{(A_2+C_1x^3)}\sqrt[3]{(C_2-A_1x^3)}}$$

$$+C_1^{1}\int_{\nu_2}^{\nu_1}\frac{dx}{x^2\sqrt[3]{(B_2+A_1x^3)}\sqrt[3]{(A_2-B_1x^3)}}$$

$$= \lambda_1\mu_1\nu_1-\lambda_2\mu_2\nu_2,$$

the limits being determined as before; and since—

$$\int\frac{dx}{yz}-\int\frac{x\,dy}{y^2z}-\int\frac{x\,dz}{z^2y} = \frac{x}{yz},$$

$$\int_{\mu_2}^{\mu_1}\frac{dx}{x^3}\,\frac{\sqrt[3]{(C_2-A_1x^3)}}{\sqrt[3]{(A_2+C_1x^3)}}+\int_{\nu_2}^{\nu_1}\frac{dx}{x^3}\,\frac{\sqrt[3]{(B_2+A_1x^3)}}{\sqrt[3]{(A_2-B_1x^3)}}$$

$$-A_1^{\frac{1}{3}}\int_{\lambda_2}^{\lambda_1}\frac{dx}{\sqrt[3]{(C_2+B_1x^3)}\,\sqrt[3]{(B_2-C_1x^3)}} = \frac{\lambda_1}{\mu_1\nu_1}-\frac{\lambda_2}{\mu_2\nu_2},$$

moreover, since

$$\int\frac{z}{x}\,dy+\int\frac{y}{x}\,dz-\int\frac{yz}{x^2}\,dx = \frac{yz}{x},$$

we shall have $\displaystyle\int_{\mu_2}^{\mu_1}dx\,\frac{\sqrt[3]{(A_2+C_1x^3)}}{\sqrt[3]{(C_2-A_1x^3)}}+\int_{\nu_2}^{\nu_1}dx\,\frac{\sqrt[3]{(A_2-B_1x^3)}}{\sqrt[3]{(B_2+A_1x^3)}}$

$$-\frac{1}{A^{\frac{1}{3}}}\int_{\lambda_2}^{\lambda_1}\frac{\sqrt[3]{(C_2+B_1x^3)}\,\sqrt[3]{(B_2-C_1x^3)}}{x^3} = \frac{\lambda_2}{\mu_2\nu_2}-\frac{\lambda_1}{\mu_1\nu_1}.$$

Again, since

$$\int dx\;y^5z^2+5\int dy\;y^4z^2x+2\int z\;dz\;xy^5 = xy^5z^2,$$

$$2A_1^2B_1\sqrt[3]{(A)_1}\int x\;dx\;(B_2+A_1x^3)^{\frac{1}{3}}(A_2-B_1x^3)^{\frac{1}{3}}$$

$$+5A_1^2C_1^2\sqrt[5]{(A_1)}\int dx\;x^4(A_2+C_2x^3)^{\frac{1}{3}}(C_2-A_1x^3)^{\frac{1}{3}}$$

$$-B_1C_1^2\int dx\;(C_2+B_1x^3)^{\frac{1}{3}}(B_2-C_1x^3)^{\frac{1}{3}}$$

$$= A_1^2B_1C_1^2\sqrt[5]{(A_1)}\,(\lambda_2\mu_2^5\nu_2^3-\lambda_1\mu_1^5\nu_1^3).$$

Similarly, since

$$\int(y+z)\;dx+\int\;(x+z)\;dy+\int\;(x+y)dz = xy+xz+yz,$$

$$\sqrt[3]{(B_1C_1)}\int\;dx\;\{\sqrt[3]{(C_2+B_1x^3)}-\sqrt[3]{(B_2-C_1x^3)}\}$$

$$+\sqrt[3]{(A_1C_1)}\int\;dx\;\{\sqrt[3]{(A_2+C_1x^3)}-\sqrt[3]{(C_2-A_1x^3)}\}$$

$$+\sqrt[3]{(A_1B_1)}\int\;dx\;\{\sqrt[3]{(B_2+A_1x^3)}-\sqrt[3]{(A_2-B_1x^3)}\}$$

$$= \sqrt[3]{(A_1B_1C_1)}\{\Sigma\lambda_2\mu_2-\Sigma\lambda_1\mu_1\}.$$

Next suppose that we transform by means of the equations—

$$a_1x+b_1y+c_1z = e_1,$$

$$a_2x+b_2y+c_2z = e_2,$$

and remember that ϕ_1, ϕ_2, ϕ_3 being any functions—

$$\int dx\, \phi_1'(x)\, \phi_2(y)\, \phi_3 z + \int dy\, \phi_2 y\, \phi_1(x)\, \phi_3(z)$$

$$+\int dz\, \phi_3(z)\, \phi_1(x)\, \phi_2(y) = \phi_1 x\, \phi_2 y\, \phi_3 z,$$

then, transforming as before (with similar limits),

$$\int dx\, \phi_1(x)\, \phi_2\frac{B_1x+C_2}{A_1}\, \phi_3\frac{C_1x-B_2}{A_1}$$

$$+\int dx\, \phi_2'(x)\, \phi_3\frac{C_1x+A_2}{B_1}\, \phi_1\frac{A_1x-C_2}{B_1}$$

$$+\int dz\, \phi_3'(z)\, \phi_1\frac{A_1z+B_2}{C_1}\, \phi_2\frac{B_1z-A_2}{C_2}$$

$$= \phi_1\lambda_1\, \phi_2\mu_2\, \phi_3\nu_2 - \phi\lambda_2\, \phi_2\mu_2\, \phi_3\nu_1,$$

we observe there are three arbitrary functions, which can be taken at pleasure, and six arbitrary constants We perceive therefore that the formula is very extensive

The limits are of course connected by the equations—

$$a_1\lambda_1+b_1\mu_1+c_1\nu_1 = e_1,$$

$$a_1\lambda_2+b_1\mu_2+c_1\nu_2 = e_1,$$

$$a_2\lambda_1+b_2\mu_1+c_2\nu_1 = e_2,$$

$$a_2\lambda_2+b_2\mu_2+c_2\nu_2 = e_2$$

Next let us transform by means of the equations—

$$a_1(x^2-2mx) + b_1(y^2-2my) + c_1(z^2-2\nu z) = e_1,$$

$$a_2(x^2-2mx) + b_2(y^2-2my) + c_2(z^2-2\nu z) = e_2,$$

then, proceeding as before,—

$$x = m+\left\{m^2+\frac{B_2}{C_1}-\frac{2rA_1}{C_1}z + \frac{A_1z^2}{C_1}\right\}^{\frac{1}{2}},$$

$$y = n+\left\{n^2-\frac{A_2}{C_1}-\frac{2rB_1}{C_1}z + \frac{B_1z^2}{C^1}\right\}^{\frac{1}{2}},$$

$$z = r+\left\{r^2+\frac{A_0}{B_1}-\frac{2nC_1^2}{B_1}y+\frac{C_1y^2}{A_1}\right\}^{\frac{1}{2}},$$

$$x = m+\left\{m^2-\frac{C_2}{B_1}-\frac{2nA}{B_1}y+\frac{A_1y^2}{B_1}\right\}^{\frac{1}{2}},$$

$$y = n+\left\{n^2+\frac{C_2}{A_1}-\frac{2mB_1}{A_1}x+\frac{B_1}{A_1}x^2\right\}^{\frac{1}{2}},$$

$$z = r+\left\{r^2-\frac{B_2}{A_1}-\frac{2mC_1}{A_1}x+\frac{C_1}{A_1}x^2\right\}^{\frac{1}{2}}$$

Hence we have the sum of the integrals (supposed to be taken within the proper limits)—

$$\int dz \left\{m^2+\frac{B_2}{C_1}-\frac{2rA_1}{C_1}z+\frac{A_1z^2}{C}\right\}^{\frac{1}{2}}$$

$$\left\{n^2+\frac{A_2}{C_1}-\frac{2rB_1}{C}z+\frac{B_1z^2}{C_1}\right\}^{\frac{1}{2}}$$

$$\cdot\int dx \left\{r^2+\frac{A_2}{B_1}-\frac{2nC_1}{B_1}x+\frac{C_1x^2}{B_1}\right\}^{\frac{1}{2}}$$

$$\left\{m^2-\frac{C_2}{B_1}-\frac{2nA_1}{B_1}x+\frac{A_1x^2}{B_1}\right\}^{\frac{1}{2}}$$

$$\int dx \left\{n^2+\frac{C}{A_1}-\frac{2mB_1}{A_1}x+\frac{B_0}{A_1}x^2\right\}^{\frac{1}{2}}$$

$$\left\{r^2-\frac{B}{A_1}-\frac{2mC_1}{A_1}+\frac{C_1x^2}{A_1}\right\}^{\frac{1}{2}},$$

expressed by elliptic functions

As a final example take the following —

Let—

$$a_1x^3+b_1y^3+c_1z^3 = e_1,$$

$$a_2x^3+b_2y^3+c_2z^3 = e_2$$

Then since

$$\int dx\, yz+\int dy\, xz+\int dz\, xy = xyz,$$

$$\sqrt[15]{(B_1^{10}C_1^{8})}\int dx\ \sqrt[5]{(C_2+B_1x^5)}\sqrt[5]{(B_2-C_1x^5)}$$

$$+\sqrt[15]{(A_1^{8}C_1^{8})}\int dx\ \sqrt[5]{(A_2+C_1x^5)}\sqrt[5]{(C_2-A_1x^5)}$$

$$+\sqrt[15]{(B_1^{10}A_1^{8})}\int dx\ \sqrt[5]{(B_2+A_1x^5)}\sqrt[5]{(A_2-B_1x^5)}$$

$$=\sqrt[15]{(A^8B^{10}C_1^{8})}(\lambda_2\mu_2\nu_2-\lambda_1\mu_1\nu_1)$$

If we put

$$\int dx\ yzw+\int dy\ xzw+\int dz\ xy\ w+\int dw\ \ xyz\ =\ xyzw$$

and transformed by three equations similar *mutatis mutandis* to those we have have used we should of course obtain the sum of four definite integrals

The limits are omitted in some of these equations but they will be easily seen from the foregoing

XXXIII "A Geometrical Interpretation of the first two Periods of Chemical Elements following Hydrogen showing the Relations of the fourteen Elements to each other and to Hydrogen by means of a Right Line and Cubic Curve with one real Asymptote By Rev **SAMUEL HAUGHTON** M D, F R S Received April 30, 1887

[Publication deferred]

XXXIV "On the Force with which the two Layers of the healthy Pleura cohere' By **SAMUEL WEST**, M D F R C P Communicated by Sir **JAMES PAGET**, Bart, F R S Received May 21, 1887

[Publication deferred]

XXXV "Total Eclipse of the Sun observed at the Caroline Islands on May 6, 1883 By W **DE W ABNEY** Capt R E, F R S. Received May 25, 1887

[Publication deferred]

XXXVI. "Note on Mr. Davison's Paper on the Straining of the Earth's Crust in Cooling." By G. H. DARWIN, M.A., F.R.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge. Received June 15, 1887.

[To be published in the 'Philosophical Transactions' in conjunction with Mr. Davison's paper.]

XXXVII. "A further minute Analysis, by Electric Stimulation, of the so-called Motor Region of the Cortex Cerebri in the Monkey (*Macacus sinicus*)." By CHARLES E. BEEVOR, M.D., and Professor VICTOR HORSLEY, F.R.S., B.S., F.R.C.S. Abstract received June 16, 1887.

[Publication deferred.]

XXXVIII. "On the present Position of the Question of the Sources of the Nitrogen of Vegetation, with some new Results, and preliminary Notice of new Lines of Investigation." By Sir J. B. LAWES, Bart., F.R.S., and J. H. GILBERT, M.A., LL.D., F.R.S., Sibthorpian Professor of Rural Economy in the University of Oxford. Abstract received June 16, 1887.

[Publication deferred.]

XXXIX. "On Diameters of Plane Cubics." By JOHN J. WALKER, M.A., F.R.S. Received June 16, 1887.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 18th.

Presents, June 16, 1887.

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Mr W Crookes, F R S

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"Note on some Experiments on the Viscosity of Ice." By J. F. MAIN, M.A., D.Sc. Communicated by Prof. W. C. UNWIN, F.R.S.

Received April 13,—Read May 5, 1887.

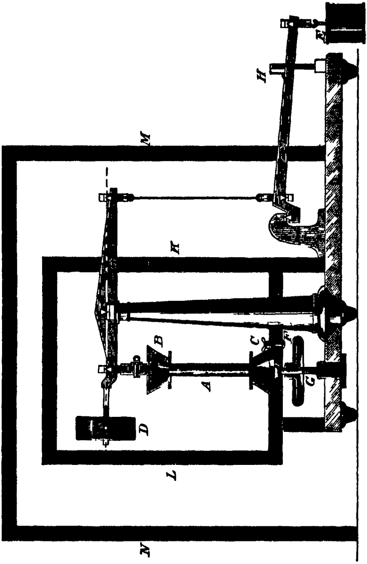
Owing to the uncertainty prevailing as to the continuous extensibility of ice under tensional stress, it appeared to me desirable to institute a series of experiments directed to this point conducted according to the methods, and as far as possible, with the exactness of modern experimental testing.

In order to eliminate the influence of regelation the experiments have been carried on at such low temperatures as preclude the possibility of any effect being produced by this cause, the highest temperature recorded in Experiment No. 1 being −2·6° C., in No. 2, −1·0° C., and in No. 3 −0·5° C. It must be remarked, moreover, that these maximum temperatures only obtained for a very short time, on one or two days, as will be seen from the records.

The testing machine which I used was constructed for me by Herr Ingenieur Usteri Reinacher of Zurich. It was on the compound lever principle, the ratio of the arms of the equivalent simple lever being 1 : 20. All parts where friction could be prejudicial were provided with knife edges. The design of the machine is obvious from the figure, in which A represents the specimen of ice to be tested, held by the collars at B and C. D is an equipoise, to balance the weights of the levers and of the vessel E through which the power is applied by means of shot. F is a hand wheel fixed to the screw G by means of which as the specimen extends, the under collar C may be lowered, so that the position of the upper collar B and of the two levers may remain the same. An index at H shows when the parts of the instrument are in the relative position required, and by its motion enables a rough estimate to be formed of the extension of the specimen.

The temperature was rendered more equable by enclosing the apparatus in two wooden boxes, KL and MN.

A delicate thermometer, graduated to tenths of a degree centigrade, and reading from −6° C. to +6° C., was attached to the central



wooden pillar, which supports the upper lever The greatest variations of temperature inside the inner box were given in Experiment 3, and, on the days mentioned in the record of Experiment 2, by two thermometers, maximum and minimum, attached to the roof of the inner box The centres of the bulbs were about 7½ cm from the roof

The ice specimen was formed by freezing water in a cylindrical iron mould, with a conical expansion at one end. To obtain ice as free as possible from included air, I in some cases (but not in all) boiled the water and then froze it. Afterwards it was melted in the mould, boiled, and then allowed again to freeze. In this way nearly all the air was expelled, only a small core of minute bubbles up the axis of the cylinder remaining. In Experiment No. 1, however, these precautions were not taken, and thus the cylinder of ice had linear bubbles of air, radiating in horizontal straight lines from the axis of the cylinder. These were due to expulsion of the air from the water, in which it was dissolved, as the latter froze in concentric cylindrical shells from outside inwards.

After the ice cylinder had been freed from contact with the mould, by bathing the latter in warm water, it was passed through the conical iron collar (C in figure) which the conical expansion of the ice fitted, and which had been screwed to the upper part of a wooden frame. The other collar (B) was then attached to a moveable plate in the wooden frame, which was capable of motion vertically and horizontally, and, by adjusting screws, both the frame and the collar B could be made accurately horizontal. By a small plummet the ice cylinder was made to hang vertically, by placing small pieces of cork between it and the edge of the opening in B. Water was then run into B, and, when frozen, a cylinder of ice was obtained held above and below in two conical collars. By the above mentioned adjustments it was ensured that the cylinder of ice was perpendicular to the surfaces of the two collars B and C.

As the weight of the two iron collars was over 4.5 kilos, much care was needed to prevent fracture of the ice, which, unless handled tenderly, broke with the least jar, on being inserted in the machine.

The lower surface of the upper, and the upper surface of the lower collar had been planed true, and brass rings fitted on them. These rings were also made plane surfaces. They extended so far from the ice specimen that it was possible to use callipers between the surfaces of these brass rings, without interference with the larger ends of the conical collars. The ring on B was provided with four small holes, 90° apart on the circumference, and by means of a small plummet the points on the ring of C (which ring was graduated roughly) that were vertically under the four points on the ring above, were determined. By noting these graduations it was rendered certain that the measures would always be taken between the same points in the upper and under rings. Since the ice specimen was inserted in the apparatus with its axis vertical, the lines measured between the points thus determined on the two rings, were, in each case, parallel to the axis of the cylinder. The callipers used measured to the one-fiftieth of a millimetre.

I used the rings because I have not been able to devise means, as yet, for so firmly fixing objects in the ice that there would be no danger of displacement by their own weight, or during the measurement by callipers. The results obtained are therefore affected by errors, if any, which may be due to action at the collars, such as slipping through, owing to distortion of the conical enlargements This effect is but slight at temperatures below the freezing point; though near and above freezing point the collars, when the ice is under tensile stress, will not hold it, but, in the course of a few hours, by distortion of its conical enlargements, it slips through them. At lower temperatures, to act as a check on the measurements taken between the rings on the collars, and to determine if any appreciable effect was due at those temperatures to slipping through the collars, I gummed two pieces of paper on the ice specimen near the top and bottom. On each of these a small pencil mark was made, and by means of a rule, I got a measure of the distance between the points. By repeating this measure on a subsequent day, a rough value was obtained of the extension between the marked points. The distance could be estimated to about the quarter of a millimetre, and showed that, with low temperatures, nearly all the extension observed was due to the stretching of the piece, and not to a shearing action of the ice in the collars.

In the tabular results of three experiments which follow, the first column gives the date; the second the hour, reckoned from Berne mean midnight, at which the observations were taken; the third column gives the temperature in degrees centigrade, read by an attached Kew-verified thermometer, graduated to tenths of a degree; the fourth column gives in millimetres the mean of the distances of the points on the upper from those on the lower ring These distances, four in number, were taken at opposite extremities of two perpendicular diameters of the rings. The next column furnishes the mean extension in millimetres in the interval that has elapsed since the last observation. The sixth column gives, in hundredths of a millimetre, the hourly mean extension deduced from the preceding column, and the interval which has elapsed between the two observations. The two next columns give, in kilogrammes per square centimetre, the mean and the maximum stress respectively. By the rapid evaporation from the ice cylinder, even at low temperatures, the amount of this stress increased from day to day, with the same load at the further end of the compound lever. Since the evaporation was different at different parts of the ice cylinder, owing probably to variations in texture, and still more to the effect of the proximity of the collars to the ice above and below, which protected the ice near them from evaporating so quickly as in other parts, the diameter of the cylinder at different heights varied. Thus the mean stress, as deduced from three measurements of the diameter, at the top in the middle, and at the bottom, differed from the maximum stress as deduced from the total load and the least area of section

The last column but one in Experiment No 1 gives the minimum temperature the preceding night measured not in the box but by a thermometer freely exposed out of doors Since the ice specimen was enclosed in the double box its temperature did not sink as low as those given in this column The numbers here are only to be taken as signifying that the night was or was not specially cold Unfortunately in this experiment no maximum and minimum temperature observations were taken In Experiments 2 and 3 the maximum and minimum temperatures were observed when recorded by a maximum and a minimum thermometer suspended in the box and their determinations furnished the range of temperature

Experiment 1

Mean original length of specimen not subjected to tension measured at 9 15 on Feb 7th	= 233 48 mm
Mean length with load of 5 5 kilos	= 234 22
Giving a sudden increase in length of	0 74
Measured just afterwards it had become	= 234 58
After 13 hours (at night) under same load	= 234 98
Giving a mean hourly extension of	0 03
After 2 hours (temp −5 0°) the length became	= 235 04
Giving an hourly extension of	0 02

The load on the specimen was then increased to 12 5 kilos, and the mean length became 235 12 mm

Date	Hour	Temperature in degrees cent grade	Mean distance of rings in millimetre	Mean extension in millimetres	Mean hourly extension in hundredths of a millimetre	Mean stress in kilos per square centimetre	Maximum stress in kilos per square centimetre	Minimum temperature out of doors preceding night	Remarks
1887									
Feb 8th	12¼	−3 7	235 12			2 7			
9th	12	−3 5	235 76	0 63	2 6			−22 0	
10th	12	−4 3	236 31	0 56	2 0			−17 9	
11th	12	−3 5	237 04	0 73	3 0			−15 0	
12th	12¼	−2 8	238 15	1 11	4 6			−13 3	
13th	11¼	−2 2	239 65	1 50	6 5			− 9 0	
14th	12¼	−4 6	240 73	1 08	4 0		4 3	−14 0	
15th	11	−5 6	241 95	1 22	5 4	4 15	4 8	−15 5	
	14	−4 2	242 15	0 20	7 0				
	16¼	−3 1	242 29	0 14	6 0				
	18¼	−3 1	242 48	0 19	10 0				
	22¼	−4 5	242 75	0 27	7 0				
16th	8¼	*β	243 45	0 70	7 0	4 3	4 9	15 0	Here it was no longer possible to screw down G
	10	β	243 52	0 07	3 0				
	12¼	β	243 63	0 11	5 0				
17th	11	β	244 45	0 82	3 5	4 3	5 2	−18 0	

Total extension of specimen − 244 45 − 233 48 = 11 mm

* Here and hereafter, β denotes a temperature below −60° C the lowest which could be read by the attached thermometer

Experiment 2

Date	Hour	Temperature in degrees centigrade	Mean distance in millimetres	Mean extension in millimetres	Mean hourly extension in hundredths of a millimetre	Mean stress in kilos per square centimetre	Maximum stress in kilos per square centimetre	Range of temperature	Remarks
1887									
Feb 19th	16¼	−4 2	236 20	0 18	3	2 08	2 1		
	22	−3 5	236 38	0 36	3				
20th	10	−2 8	236 74	0 10	[illegible]	2 1	2 2		
	14¾	−2 2	236 84	0 09	4				
21st	12	β	236 93	0 13	5	2 2	2 28		
22nd	12	β	237 06			2 23	2 35	−2 0 to −3 0	
23rd	11¼	β	237 14	0 08	3	[illegible]8 3 65	2 39 3 8	−2 0 to −6 5	A greater stress was here put on the specimen
24th	9¼	−2 6	237 83	0 69	3			−[illegible] 1 to −5 0	
	12¼	−1 7	238 02	0 13	6			1 0 to −2 0	

Total extension of specimen 238 02 − 23 20 1 82 mm

Experiment 3

Date	Hour	Temperature in degrees centigrade	Mean distance in millimetres	Mean extension in millimetres	Mean hourly extension in hundredths of a millimetre	Mean stress in kilos per square centimetre	Maximum stress in kilos per square centimetre	Range of temperature	Remarks
1887 Feb 27th	11	−2·4	215·69			0·00	0·00		Specimen without load
				0·22					
			215·91			0·85	0·85		Load of 5·5 kilos
				0·04					
			215·95			1·9	1·9		Load of 12·5 kilos
				0·25	8·0				
	14	−2·8	216·20					1·0 to −3·0	
				0·39	1·9				
28th	11½	−3·7	216·59					−1·0 to −4·3	
				0·40	1·8				
Mar 1st	10	−3·8	216·99			1·95	1·98	−1·0 to −3·7	
				0·20	2·2				
	19	−0·7	217·19					−0·5 to −2·7	
				0·14	1·0				
2nd	9½	−4·0	217·33			2·0	2·08	−0·5 to −4·0	
				0·08	1·0				
	17	−0·6	217·41						

Total extension of specimen = 217·41 − 215·69 = 1·72 mm

The specimen broke by a jar due to the falling of a weight on the floor of the room

On February 27th the three observations were taken, with different loads on the specimen, directly after one another With a load of 5 5 kilos (the weight of the lower collar) there was an immediate extension of 0 22 mm , and when the load was increased to 12 5 kilos, the extension at once increased by 0 04 mm

The three experiments, of which the results are given above show that ice subjected to tension stretches continuously by amounts which evidently depend on the temperature and on the tensile stress When the stress is great, as in No 1, and the temperature not very low, there are appreciable extensions, as on February 15th, amounting to as much as 1 per cent of the whole length per day When the temperature is lower, and the stress is less, the extension is less, but still such as can be measured So continuous and definite is the extension, that it can even be measured from hour to hour, as seen in Experiment No 1, when for February 15th and 16th easily measurable extensions were obtained for intervals of two and three hours The quantities actually measured were generally both notably greater and less than the mean since owing probably to inequalities in the distribution of the stress and to variations of texture in the ice, due to internal strains produced in freezing one side of the specimen would sometimes stretch 50 per cent more than the other Hence differential motions resulted in the ice Those motions and extensions took place at temperatures which preclude all possibility of melting and regelation, especially in Experiments 1 and 2 In Experiment No 1, it was found that in three days, from February 13th to February 16th, the distance between two marks on pieces of paper gummed on the ice increased from 200 mm to 203 mm , giving an elongation of $\frac{1}{2}$ per cent per day This method fails at temperatures very near the freezing point, owing to the danger that by thawing the pieces of paper may slip, but is free from this risk at lower temperatures It is in any case very rough, and is only useful as a check on the other measures since there is no question here of slipping through the collars How close the correspondence in the results obtained is may be seen by observing that in Experiment 1 there was an extension in nine days of 11 mm in the case of a cylinder of ice, which at the beginning of that time was 235 mm long, and this gives an elongation of just about $\frac{1}{2}$ per cent. per day, the same as resulted from the three days' observation with the marked bits of paper

The experiments are to be regarded rather as proving the existence of continuous extension under tensile stress than as determining its amount The quantities observed are functions of the stress, of the time between two observations, and of the time integral during that interval of the temperature Owing to the rapid evaporation from ice, even at low temperatures, when the surface is never liquefied, at such an elevation (6100 feet above sea level) as St Moritz, in the Engadine where the observations were carried on, the specimen is subjected to a continually increasing stress This variation in the stress can no doubt be diminished by clothing the specimen in flannel The change of temperature I expect to diminish by filling the interval between the two surrounding wooden boxes with some non conducting material In this way I have good hope that, on resuming the experiments next winter at St Moritz, I may be able to determine more nearly the law of extension That there is such extension, and that it goes on continuously with all stresses above 1 kilo per square centimetre, and at all temperatures between −6° C and freezing point, is shown by the above experiments When ice is in a condition such that pressure with the point of a needle will cause a set of radiating fractures to pass from the point of contact in all directions, it stretches as certainly, although not by so great an amount as when it will permit the passage through it of the same needle without showing the least trace of flaw or scar

In the discussions, for the most part *a priori*, on the extensibility of ice, sufficient importance has not usually been assigned to the necessity of distinguishing between the effect of even a small blow or jar and that of a much greater force applied gradually and steadily during a long interval A bar of ice may bear a stress of 4 and 5 kilos per square centimetre if the load is steady, which would fracture at once with a much smaller sudden stress, especially if not uniformly distributed

In the first experiment we notice a total extension in nine days of 11 mm In No 2 there is an extension of 1 8 mm in five days, and in No 3 of 1 7 mm in three days If we assume for the moment an extension proportional to the time, we should thus get a mean daily extension in the three experiments of 1 2 mm, 0 36 mm, and 0 56 mm respectively To account for the discrepancy we remark that the stress in No 1 is much greater than in 2 or 3, and the temperature not so very low during the day, although low at night The effect of increased stress is well shown in Experiment No 2, on February 23rd and 24th, where, on increasing the stress from 2 28 to 3 65 kilos, the extension in a day rises at once from 0 08 to 0 69 mm In No 2, for three out of the five days, the temperatures were below −6° C, whilst in No 3 there was a low stress but comparatively high temperatures

In Experiment No 2 the large numbers obtained at first probably arose from the fact that in preparing the specimen its conical expansion had frozen to the collar C When the water run into the collar B began to freeze it expanded, and thrust the ice upwards

As it was frozen to the collar C, and therefore unable to expand upwards, since both collars were fixed to a frame, the cylindrical part of the ice bulged outwards, as in the usual case of a long column under compression. When the specimen was subsequently exposed to tension, the effect of the latter was to straighten it, so that in a few days it no longer bulged out. The straightening of the central line of the ice cylinder thus gave rise to greater extensions than were due simply to the extension of a straight bar of ice with an equal distance in all azimuths between the rings

"The Air of Sewers." By Professor THOMAS CARNELLEY, D.Sc., and J. S. HALDANE, M.A., M.B., University College, Dundee. Communicated by Sir H. ROSCOE, F.R.S. Received May 21.—Read June 16, 1887.

Owing to the complaints which had been made of bad smells in the House of Commons, a Select Committee was appointed in the spring of 1886 to inquire into the ventilation of the House In consequence of the experience we had gained in the course of an extensive examination of the air of houses and schools in Dundee (see 'Phil. Trans.,' vol. 178 (1887), B, p. 61), we were instructed by the Committee to make a series of analyses of the air in the sewers under the Houses of Parliament, and to report thereon (see 'Second Report of the Committee,' Appendix). Since then we have examined the air in a considerable number of sewers in Dundee.

Our object was, in the first place, to obtain a general idea of the amount of some of the more important impurities present in sewer air. But we have also endeavoured to throw some light on their sources, and on the conditions affecting their dissemination. With this view we found it desirable to supplement our observations in the sewers by a certain number of laboratory experiments.

In spite of the great amount of discussion which has taken place in connexion with real and supposed danger from sewer air, there have hitherto been but few analyses published of the air of sewers of

The first and most complete set of analyses was that made by Dr. Letheby in 1857–58 ('Report to the City of London Commissioners of Sewers,' 1858). He examined the air of thirteen sewers in the City of London. The following are the means of his analyses:—

Oxygen per cent	Nitrogen per cent	Vols of carbonic acid per 10 000	Marsh gas and sulphuretted hydrogen	Ammonia	100 grains of air deprived of water and carbonic acid gave after oxidation	
					Carbonic acid	Water
19 506	79 962	58 2	traces	Rather abundant	1 247 grains	1 126 grains

Unfortunately no information is given as to the condition and means of ventilation of these sewers

In 1867 Dr Miller,* in an investigation on the action of charcoal air filters, made a number of analyses in two London sewers The first series was made in a clean and well ventilated sewer, and the second in a sewer described as "tide locked and ill ventilated In the first series (eighteen analyses) he found on an average 10 6 vols and in the second (six analyses) 30 7 vols of carbonic acid per 10 000 vols of air In neither series could sulphuretted hydrogen be detected, and in both series the ventilation was by means of open gratings

In 1877 Beetz† in Munich found 31 4 vols of carbonic acid and 2 2 vols of ammonia per 10 000 as an average of five analyses

As regards the micro organisms present in sewer air the only analyses hitherto published are those of Miquel ‡ He says, The atmosphere of sewers always saturated with moisture and constantly in contact with water more or less filthy and loaded with putrefying substances, is heavily charged with bacteria Judging from a series of experiments made in the sewer of the Rue de Rivoli in the neighbourhood of the point at which this sewer joins the large collector of the Boulevard Sebastopol there are present in the air circulating in this gallery 800 to 900 bacteria per cubic metre '§ (= 0 8 to 0 9 per litre) He also states that the air of the sewer contains an almost constant number of bacteria, and that in summer the air of the Rue de Rivoli may exceed in impurity by five or six times that of the sewer, while in winter the air of the sewer may be five or six times more impure No details are furnished as to the condition and means of ventilation of the sewer nor as to the number of analyses on which these conclusions are based

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* 'Chemical News' March 13th 1868

† Quoted by Erismann in Pettenkofer and Ziemssen's Handbuch der Hygiene,' vol 2, p 197

‡ 'Les Organismes Vivants de l Atmosphère,' 1883 p 278

§ These numbers refer to the bacteria capable of developing in a solution of Liebig's extract of 1·024 specific gravity placed in an incubator at 30°—35°

Miflet ('Biedemann's Centralbl,' 1880, p 227) states that air taken from above a sink was rich in micro organisms

In detailing our own observations it may be as well, in the first place, to give some account of the sewers in which they were made The main sewer of Westminster Palace,* in which our first observations were made, ran along underneath the open courts in the centre of the building from the neighbourhood of the Victoria Tower to that of the Clock Tower a short way beyond which it joined the main low level metropolitan sewer Along its course it varied irregularly in height from 4½ to 10½ feet It was ventilated by suction from the large furnace at the foot of the Clock Tower, and was cut off by a penstock from the metropolitan sewer The air drawn into the shaft of the furnace almost all came from openings near the Clock Tower end of the sewer In the rest of the sewer there were no open gratings, and the draught was feeble On opening the trap door of a man-hole near the Victoria Tower there was an upward rush of air and condensed vapour, in spite of the very powerful suction at the other end of the sewer The sewer was flushed daily It was clean, and the flow of sewage was pretty rapid The water frequently accumulated in the sewer during rain owing to rise of the level of the water in the metropolitan sewer Our analyses were made when the water was escaping freely

The second set of analyses at Westminster was made after the ventilation of the sewer had been altered by carrying an additional shaft to the furnace and placing inlet gratings in suitable positions By this means the draught along the sewer from the neighbourhood of the Victoria Tower was much increased

We owe the facilities which were afforded us for examining the air in the Dundee sewers to the courtesy of Mr Mackison, Burgh Engineer We met with a variety of conditions in these sewers They are all ventilated by open gratings placed in the roadway at distances of about 50 yards apart, and also by the open drain grids placed along each side of the road at distances of about 40 yards The flow of sewage was in almost every case pretty rapid at the time when our analyses were made The sewers in Commercial Street, Overgate, and Nethergate are egg shaped, that in Murraygate is a large circular sewer Those in Reform Street and Dock Street had originally been large, old, flat bottomed stone sewers, but had been partially altered by the substitution of a bottom similar to that of an egg-shaped sewer The Dock Street sewer is alternately filled and emptied by the tide, as is also the sewer in Commercial Street, opposite Exchange Street The approximate heights of the several sewers are

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* This sewer has now (1887) been replaced by a pipe, on the recommendation of the Select Committee

4 Main Sewer under Houses of Parliament

Date and place	Time	Approximate height of sewer	Temp F	Vols CO$_2$ per 10 000 vols of air	Vols of oxygen required to oxidise the organic matter in 1 000 000 vols of air	Total No of micro-organisms per litre of air
April 19th 20th before alterations						
First series —		feet				
In sewer near Victoria Tower	5 0 P M	4½	55°	8 9	9 5	1 (0)
Ditto near kitchen	6 0	7	59	8 6	12 9	2 (1)
Ditto near Clock Tower	7 30	4½	56	7 6	12 8	13* (0)
Outside air in centre of court near old Crown Office	8 30		51	4 2	3 1	9 2*(—)
Second series —						
In sewer near Victoria Tower	10 30	4½	52	8 8	9 5	2 (0)
Ditto near kitchen	11 15	7½	56	7 3	9 5	5 (0)
Ditto near Clock Tower	12 midnight	4½	52	5 4	11 9	19 (8)
Outside air at same place as before	1 0 A M		47			4* (0 4)
May 18th 19th after alterations						
Third series —						
In sewer near Victoria Tower	5 0 P M	4½	56	7 3	5 4	4 (1)
Ditto near kitchen	5 20	7	58 5	5 9	3 2	8 (4)
Ditto near Speaker's Court	7 10	10½		6 2	3 1	
Ditto near Clock Tower	7 20	4½	57		3 2	38 (1)
Outside air at fresh air inlet to sewer	6 0		59	4 3	2 5	18 (4)

A Main Sewer under Houses of Parliament—*continued*

Date and place	Time	Approximate height of sewer	Temp F	Vols CO₂ per 10 000 vols of air	Vols of oxygen required to oxidise the organic matter in 1 000 000 vols of air	Total No of micro-organisms per litre of air
Fourth series —		feet				
In sewer near Victoria Tower	11 0 P M	4½	56°	6 7	1 0	0 5 (0)
Ditto near kitchen	11 40	7	59 5	4 9	1 6	2 (1)
Ditto near Speaker's Court	1 15 A M	10½		6 7	Too small to estimate	
Ditto near Clock Tower	1 30 „	4½	56	5 8	1 7	8 (2)
Outside air at fresh air inlet to sewer	12 30		47	4 5	Too small to estimate	3 (1 5)

ers

April 27th under C			
In sewer between Murrayg		15	(2)
Ditto		15	(0 5)
In sewer opposite Excl ang	treet	30	5 (1)
Outside air in open yard C	mercial Street	45	4 (1)
April 29th under Murray	te and High S		
In sewer near entrance of I	Town Sewer	30	(0)
Ditto near Meadow entry		30	
Ditto near Pillars		45	
Outside air in open yard C		15	(0 2)

B. Dundee Sewers—*continued*

Date and place	Time.	Approximate height of sewer	Temp. F	Vols. CO₂ per 10,000 vols. of air.	Vols of oxygen required to oxidise the organic matter in 1,000,000 vols. of air.	Total No. of micro-organisms per litre of air.
May 7th.		feet				
In sewer under Overgate.	4.15 P M	3 5	53°	8 6	18 2	13 (3)
Ditto under Reform Street, High Street end	5 0 ,,	7	52	7·4	7 6	14·5 (0 5)
Ditto, ditto, Albert Square end	6 0 ,,	7	54	10 8	6·7	4 (0)
Ditto under Nethergate	7.30 ,,	4	..	10 9	6·4	25 (1)
Outside air in open yard, Commercial Street	8.30 ,,	..	..	3 0	2 2	16 (1·2)
May 11th, under Dock Street.						
In sewer near Sailors' Home	2.50 ,,	9	52	7 9	8·1	6 (0)
Ditto east of entrance to Commercial Street	3 50 ,,	7	53	6 1	9 6	[103 (0)]†
Ditto opposite entrance to Commercial Street	4.15 ,,	7	..	5 5	3 8	12 (2)
Ditto near foot of Union Street	5.0 ,,	5	49	9 4	9 1	12 (2)
Outside air in yard off Dock Street	4.30 ,,	..	46	3 1	3 1	60 (—)

The figures in brackets placed beside the figures for total micro-organisms refer to the number of moulds per litre.

* An asterisk signifies that owing to running *together of the* colonies in the Hesse's tube the total number of micro-organisms could not be found correctly. The true number must have been larger than that actually given.

† Not included in the averages, as the highness of the number was due to exceptional circumstances (see below).

		Total				In excess of outside air at time			
		Temp F	Vols carbonic acid per 10 000 vols of air	Vols oxygen to oxidise the organic matter in 1 000 000 vols of air	No of micro organisms per litre	Temp F	Vols carbonic acid per 10 000 vols of air	Vols oxygen to oxidise the organic matter in 1 000 000 vols of air	No of micro organisms per litre
April 19th to May 19th, 1886	In sewers	54	7 5	7 2	8 9	5 2°	3 8	4 9	—7
	Outside air at same time	49	3 7	2 2	15 9				
Nov 28th 1885 to end of April 1886	Houses —								
	One roomed	55	11 2	15 7	60	19	6 6	6 2	60
	Two roomed	53 5	9 9	10 1	46	18	5 5	2 2	46
	Four rooms and upwards	54 5	7 7	4 5	9	14	3 3	1 4	9
	Schools —								
	Naturally ventilated	55 6	18 6	16 2	152	16 8	15 1	7 8	152
	Mechanically ventilated	62	12 3	10 1	16 5	24	8 9	1 1	16 5

given in the table of results (pp 504—507) It will be seen that the sewers which we examined were all of considerable size, large enough to be entered without great difficulty Our data, therefore, do not apply to small sewers and drains

In each sewer examined we estimated simultaneously the amounts of carbonic acid 'organic matter,' and micro organisms, as in the case of our observations referred to above on the air of houses and schools Analyses of outside air in the immediate vicinity of the sewers examined were made at as nearly as possible the same time In order to avoid as far as possible contaminations due to our own presence we kept to leeward of the apparatus employed in collecting the samples The methods employed were that of Pettenkofer for carbonic acid, Carnelley and Mackie s modification of the permanganate process for organic matter * and Hesse s method for micro organisms † The results obtained are given in the preceding table

For the purpose of giving a general idea of the relative impurity of sewer air we have taken the averages of analyses A and B of sewer air and placed them alongside of the averages for outside air at the same time and for various classes of houses and schools, as determined by us in the winter of 1885–86 and detailed in the paper mentioned above

The above table shows (1) that the air of the sewers was much better than one might have expected (2) that the carbonic acid was about twice and the organic matter rather over thice times as great as in outside air at the same time whereas the number of micro organisms was less (3) that in reference to the *quantity* of the three constituents named the air of the sewers was in a very much better condition than that of naturally ventilated schools and that with the notable exception of organic matter it had likewise the advantage of mechanically ventilated schools, (4) that the sewer air contained a much smaller number of micro organisms than any class of house The carbonic acid was rather greater than in the air of houses of four rooms and upwards, but less than in two and one roomed houses As regards organic matter, however, the sewer air was only slightly better than the air of one roomed houses, and much worse than that of the other classes of house ‡ These facts are brought out more clearly in the following table, in which the average quantity in excess of outside air of each constituent in sewer air is taken as unity

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* 'Roy Soc Proc, vol 41 p 238

† 'Mittheilungen aus dem k Gesundheitsamte,' vol 2 p 182

‡ The data for all the classes of houses refer to sleeping rooms when occupied during the night

		Carbonic acid	Organic matter	Micro organisms *
Sewers		1	1	1
Houses	one roomed	1 7	1 3	7*x*
	two roomed	1 4	0 45	5*x*
	four rooms and upwards	0 [illegible]	0 3	*x*
Schools	naturally ventilated	4 0	1 6	17*x*
	mechanically ventilated	2 3	0 2	2*x*

On comparing the average amount of carbonic acid found by us in sewer air with that found by earlier investigators (see above) it appears that the sewers we examined must have been much better ventilated than those previously examined

In our paper on the air of schools and houses we pointed out that in individual cases the amount of carbonic acid is not a measure of the amount of the organic matter and number of micro organisms present in the air at the same time and that it is only when the average of a comparatively large number of cases is taken that the organic matter is seen to increase with the carbonic acid while the micro organisms show no evident connexion with the carbonic acid and organic matter In the air of sewers the relation to one another of carbonic acid and organic matter is similar The micro organisms on the whole decrease as the other constituents increase This is shown in the following table in which the organic matter and micro organisms are compared in amount with the carbonic acid as a standard The table is constructed by dividing all the carbonic acid determinations into three equal groups according to the amounts of carbonic acid found and then taking the average of the corresponding organic matter and micro organism determinations in each group

	Tempe rature	Carbonic acid	Organic matter	Micro organisms
Total —				
4 9— 6 2 vols carbonic acid	55 8°	5 7	5 1	8 7
6 7— 7 9	59 1	7 3	6 3	6 4
8 6—10 9	58 0	9 4	10 5	5 4

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* In this case we have represented the relation of the number for sewer air to that for air in four roomed houses by *x* as the calculated number for sewer air is negative The real value of *x* must be between [illegible] (= 1 8) and infinity

	Temperature	Carbonic acid	Organic matter	Micro organisms
In excess of outside air at same time —				
0 4–2 5 vols carbonic acid	7 0°	1 76	2 2	– 6 0
2 8–4 1	5 5	3 6	6 3	– 2 9*
4 7–7 9	4 5	6 0	6 8	–18 2

Sources of the several Impurities in Sewer Air

The source of organic matter present in sewer air over and above that present in outside air at the time is of course the sewage itself The organic matter arising from the sewage is most probably wholly or for the most part gaseous for the conditions which cause the number of micro organisms in sewer air to be less than in outside air would also affect any solid organic matter or dust in a similar manner so that we should expect the solid organic matter in sewer air to be less than in outside air at the same time The gaseous organic matter arising from the sewage itself will probably be of two kinds, that volatile *per se* and that volatile with the aqueous vapour from the sewage water Organic matter may also get into the air in cases where splashing occurs from the entry of a side drain high up in the wall of a sewer

The carbonic acid in sewer air over and above that in outside air may have two sources It may be due to diffusion into the sewer from the neighbouring soil but probably its chief source is oxidation of organic matter in the sewage and the air of the sewer Miller many years ago demonstrated the existence of such oxidation in the Thames when it was the recipient of all the London sewage He showed by a series of analyses of the gases dissolved in Thames water collected at various points above and below London, that from Kingston to Greenwich the carbonic acid increased, while the oxygen rapidly diminished

Two possibilities occur as to the source of the majority of the micro organisms in sewer air They may in the first place be derived from the sewage and sewer walls If this were so to any great extent we should expect their numbers to increase with the length of time during which the air is present in the sewer, and therefore with increase in the carbonic acid. But it has been shown above (p. 509) that the very opposite is the case, the micro-organisms becoming less numerous with increase of carbonic acid. We have also classified the whole of our observations according to whether they were made at points where there was a strong, moderately strong, or very feeble draught. Here again it will be seen that the results are against the theory that the micro-organisms come from the sewer itself.

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* The irregularity here is due to the results in the Dock Street sewer, which was examined on a dry and windy day when the micro organisms in the outside air were very numerous owing to dust so that the difference between outside air and sewer air was very great (see the large table of results p 506) In the first classification one of the Dock Street analyses falls into each class in the second classification two fall into the third class leaving none in the second class If the Dock Street analyses were left out or if the third class began at 4 9 vols of carbonic acid instead of at 4 7 the decrease in micro organisms would run quite regularly

	Carbonic acid	Organic matter	Micro-organisms.
Strong draught	6 6	5·7	9·9
Moderate draught	7 5	8 8	8·9
Little or no draught..............	9 4	8·1	6 7

	In excess of outside air.		
	Carbonic acid.	Organic matter.	Micro-organisms.
Strong draught..................	2·6	3 5	− 2 3
Moderate draught............	3 9	6 6	− 9·2
Little or no draught........... ...	6·0	5·5	−14 3

A similar result was obtained from our observations at Westminster, where we made six observations before, and six after the improvements in the ventilation referred to above.

	Carbonic acid.	Organic matter.	Micro-organisms.
Average before improvement........	7 8	11 0	7
Average after improvement.........	6·2	2·7	10·3

The only other source for the micro-organisms of sewer air is contamination from the outside air. The same arguments which have just been applied against the sewer itself being a source of micro-organisms may be urged in favour of their origin from outside air.

The mere fact that the average number found in the sewer air (8 9) was less than that in outside air at the same time (15 9) is itself a strong argument in favour of the origin of most of the micro-organisms from outside air If the air takes up micro organisms in its course along a sewer, we should expect the number to increase rather than diminish during its passage, whereas the opposite is the case, doubtless from gradual settling of solid particles This settling is perhaps even greater than appears from our analyses, as it was not practicable to take specimens of outside air at the gratings in the centre of the roadway with the traffic proceeding as usual At these points the contamination of the air by solid particles of organic origin would of course be at its maximum

It will be noticed that in the analyses made at Westminster the numbers obtained for the sewer air close to the Clock Tower were always larger than those for outside air (see Table, pp 504—505) Not much stress can, however be laid on this fact, as a great part of the air passing along the sewer at this point came from a side drain near the Clock Tower, leading from a point where the outside air was much more likely to be contaminated by dust from traffic than in the central court where the outside air analyses were made The outside air determinations at Westminster apply strictly to the sewer determinations near the Victoria Tower and kitchen, as these determinations were made just at the opening of the inlet grating ventilating this part of the sewer It will be seen that the micro organisms inside the sewer decreased in proportion to the decrease in those present in the outside air

Another argument in favour of the origin of most of the micro-organisms from outside may be derived from the fact that the average proportion of moulds to bacteria was nearly the same in the sewer air and corresponding outside air, 1 to 9 in the former and 1 to 8 in the latter Were the micro organisms in sewer air mostly derived from a different source than outside air we should expect the proportion to be different Thus in two cases referred to below, in which the micro organisms were evidently derived from splashing in the sewer, among 128 micro organisms there were no moulds In the micro organisms present in the air of naturally ventilated schools and one roomed houses, the average proportion present was found to be 1 132 and 1 49 respectively, as against 1 2 5 in the corresponding outside air ('Phil Trans 1887, B, p 99)

A final argument is that so far as a naked eye examination of the colonies allowed one to judge, the micro organisms in the sewer air we examined were, with perhaps one exception, similar to those in outside air The exception referred to was in the case of some very rapidly liquefying colonies which occurred in several samples of sewer air, collected at points where there was more or less splashing

These possibly came from the sewer itself, as we have not observed in outside air or in buildings any colonies which liquefied the jelly as rapidly or so extensively

The conclusion thus arrived at as to the source of most of the micro organisms present in sewer air is perhaps, at first sight, contrary to what one might have expected It is in agreement with the fact that the state of cleanliness or filthiness of a sewer seems to have no perceptible effect on the number of micro organisms present in the air of the sewer Thus two observations on two different days and at two different points of the dirtiest sewer we examined, gave only 2½ and 12 micro organisms respectively, as compared with an average of 4½ and 9 in other and cleaner sewers on the same days Our conclusion is also in agreement with what is known as to the distribution of bacteria in air Nageli ('Die Niederen Pilze, pp 109 111) has shown that liquids or damp substances do not, with ordinary air currents give off micro-organisms to the surrounding air He even found that air drawn through gravel which had been saturated with filth and then dried gave off no micro organisms (p 109) Miquel (Comptes Rendus ' vol 91 p 64) states that the vapour of water rising from the soil from rivers or from masses in full putrefaction is free from germs that the gases evolved from decaying substances and the air passed over putrid meat are free from germs provided that the putrefying substance is as moist as soil taken 0 3 metre from the surface The experiments of Professor Frankland (Roy Soc Proc vol 25 1877, p 542) also point to the improbability of micro organisms being disseminated in air by such agitation of a liquid as that produced by the flow of sewage along a sewer On the other hand it is well known that the micro organisms already present in air are always tending to sink to the ground On this fact Hesse s method depends Hence air in its passage along a sewer will presumably tend to gradually deposit its micro organisms especially if the air current is slow

In order further to elucidate this point, and in particular with regard to the drain pipes leading into houses we made some experiments with an artificial drain pipe Through the side of a wooden box, AB, there was passed the end of a piece of glass tubing CD, 5 feet long and 1¾ inches in diameter, and open at both ends In the opposite side of the box there was a hole by means of which the air inside the box could be connected with the entrance to a Hesse s tube, and the micro organisms thus determined Through the roof of the box there passed a chimney in which a draught was maintained by means of a small flame F kept burning at the bottom This, of course, caused a corresponding draught through the long tube and into the box A constant stream of water was kept running along the

bottom of the experimental sewer the sides of which were also moistened before each experiment. By estimating the micro organisms in the air at the mouth of the tube and in the box the difference caused by passage of the air along the tube could be determined. The rate of the current of air through the long tube was in all the experiments 5 feet in six seconds. The determinations were made simultaneously after the draught had been established for a short time.

No of experiment	Quantity of air aspirated through Hesse's tube	Air of laboratory (before passing through tube)		Air of box (after passing through tube)	
		Total micro organisms	Moulds	Total micro organisms	Moulds
1	½ litre	200		100	
2	½ litre	205		141	
3	5 litres	4	0	3	1
4	5 litres	3	0	2	1
5	5 litres	1	1	1	0
6	½ litre	344	17	130	42
7	½ litre	2[illegible]1	67	175	73
	Average =	111	23	80	23

In Nos 1, 2, 6, and 7 the air was rendered dusty by shaking mats in the room. Nos 3, 4, and 5 were made with the air of the laboratory in its ordinary condition.

It will be seen that the micro-organisms were diminished by nearly one half in passing along the tube This confirms our conclusions as to the settling of micro-organisms in sewer gas The micro-organisms would settle out of a drain-pipe especially with great rapidity Judging from the rate at which they settle in a Hesse's tube, air standing in, or passing along, a 4-inch drain-pipe would become entirely free of micro-organisms within three or four minutes Hence it seems improbable that micro organisms can penetrate into a house from a sewer unless with a pretty rapid current towards the house

It will be seen from the table that the moulds are more numerous in proportion to bacteria after the air has passed through the tube than before This is due to the fact, first observed by Hesse, that moulds fall through air less rapidly than bacteria We should expect to find a similar alteration in the proportion in badly ventilated sewers, but our observations in such sewers were not sufficiently numerous to enable us to say whether this is actually the case

Although, as has been seen, most of the micro organisms present in the air of the sewers we examined seem to have come from the outside air, yet in some cases we had distinct evidence of the dissemination of micro organisms from sewage itself In Dundee a few, and at Westminster a large proportion of the drains were found to enter the sewers through the roof This gave rise to a considerable amount of splashing, the effect of which on the dissemination of micro-organisms in the air it seemed of great importance to investigate The following observations in the sewers bear upon this point An analysis was made within about 2 feet of a shower of water proceeding from the roof of the Dock Street sewer, the draught being very slight The number of micro organisms present was 103 (all bacteria) An analysis made shortly afterwards a few feet to windward of the shower of water gave only twelve micro organisms During one of the analyses made at Westminster, a sudden and very violent shower of sewage occurred about 10 feet to windward of the tripod carrying the Hesse's tube In this case the number found was 25 (all bacteria), whereas an analysis made at the same point a few minutes later after the dripping had ceased, gave only eight micro organisms One of the analyses in the Murraygate sewer was made within about 30 feet of the point where the Hill Town sewer enters the Murraygate sewer there being a draught of about 2 feet per second from this point to the spot where the analysis was made The Hill Town sewer has a steep incline, and the water contained in it rushes down with great force, forming a sort of water-fall, the roar of which sounded most impressive as it echoed along the sewer The analysis only gave three micro organisms per litre *

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* The low number thus obtained was possibly owing to the fact that the waste

From the first two observations it appears that micro-organisms are undoubtedly disseminated in sewer air by splashing; but whether they are carried far in the air cannot be decided from the above experiments. The point is one of great practical importance, as the micro-organisms in question are those on which most suspicion of properties injurious to health naturally falls. Hence we thought it desirable to make some laboratory experiments with a view to elucidating the matter.

In connexion with the effects of splashing we also investigated the effects of the bursting of bubbles. Professor Frankland ('Roy. Soc. Proc.,' vol. 25, p. 542) has already made experiments on this point by means of lithia solutions. He found that lithia was disseminated in the air and carried to a considerable distance, when a solution of lithia was made to effervesce. Hence the presumption is that micro-organisms might be disseminated in a similar way

Our experiments were made with the artificial drain-pipe arrangement described above (pp. 513—514). Control determinations of the air in the box were first made after the draught had been established some little time. A putrefying solution was then poured from a height into a vessel placed at about 6 inches below the end of the glass tube, so as to imitate the splashing in a sewer; or effervescence was brought about in the same solution, placed at the mouth of the long glass tube by adding sodium carbonate and hydrochloric acid, or by blowing small and numerous jets of air through the putrid fluid by means of a fine rose from an ordinary garden hose pipe

water from a dye works was discharged into this sewer, accompanied by a distinct smell of chlorine at the time of our experiment These conditions possibly exerted a disinfecting action

No. of experiment.	Mode of producing splashing or effervescence.	Quantity of air aspirated.	Air of box after passing through drain-pipe.		Remarks.
			Without splashing, &c.	During splashing, &c.	
Series I.					
1..........	By addition of hydrochloric acid and sodium carbonate to putrid fluid.	900 c.c.	0	2	The lowness of the numbers obtained during effervescence in this case was possibly due to the disinfecting action of free hydrochloric acid There were no moulds in either case.
2..........	Ditto.........................	900 c c.	0	4	
Series II.					
3..........	By pouring putrid matter from a height	1 litre	1	About 370	On second day, after which they were too numerous to be counted On the 5th day 300 moulds were counted, but the bacteria were too numerous.
4..........	Ditto....	1 litre.	1	About 380	Ditto About 100 moulds on 5th day
Series III					
5..........	By blowing air through putrid fluid.	1 litre.	1	About 600	Ditto. About 100 moulds on 5th day.
6..........	Ditto.........................	1 litre.	1	About 500	Ditto. About 143 moulds on 5th day, one being like the large ones in 7th experiment.

No of experiment	Mode of producing splashing or effervescence	Quantity of air aspirated	Air collected directly over and about 9 inches above the putrid matter		Remarks
			Without splashing &c	During splashing &c	
Series IV 7	By pouring putrid matter from a height	1 litre	0	About 380	On 2nd day after which they were too numerous to be counted On 5th day there were 38 moulds of which six were large beautiful white interlacing colonies throwing up delicate hair like threads, about an inch long and bearing minute black fruit heads at their extremities

These results are very decided, and confirm and extend for micro-organisms the results obtained by Professor Frankland for lithia solutions They show conclusively not only that micro organisms are disseminated in sewer air by splashing but that those having this origin may be carried to a considerable distance along a sewer or drain pipe Calculating from these experiments, air vitiated as above described, and to a similar extent would still contain about 400 micro organisms per litre after travelling about 60 yards, in a sewer 5 feet high and with a draught of about 1 foot per second It is therefore of the greatest importance that sewers and drains should be so arranged as to avoid splashing as much as possible

The Physiological Effects of Unorganised Organic Matter in Sewer Air

In view of the fact that ordinary sewer air in the absence of splashing, turned out to be to all appearances comparatively innocent as regards its micro organisms and assuming that it has an injurious effect on health, we directed further attention to the unorganised organic matter present in it Of organic compounds most likely to produce some of the bad effects ascribed to sewer air, volatile ptomaines* at once suggest themselves on account of the intensely poisonous properties possessed by various known ptomaines † We therefore endeavoured to ascertain whether sewer air contains any poisonous volatile bases For this purpose air was drawn continuously for thirty four days from the sewer side, below the trap, of an earthen pipe, which acted as the drain from the College water closets and urinals This air was bubbled continuously through very dilute sulphuric acid, in order that any basic substance which the air contained might be retained The solution thus obtained was subsequently neutralised exactly with ammonia, and evaporated to dryness on a water bath The residue was dissolved and injected subcutaneously into rabbits, but produced no effect whatever, even in doses of a gram of the dry substance Evidently if there was any poisonous substance in the air, it was not contained in the residue injected Unfortunately this experiment is not conclusive, on account of the instability of many of the organic bases in question

If poisonous organic substances had been present in serious quantities in the air of the sewers we examined, we should presumably have ourselves felt some effects from them, as we were sometimes in the sewers for several hours, more or less continuously We could never observe any bad effects, however, from our stay, although we were previously quite unaccustomed to entering sewers

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* Ptomaines are basic nitrogenous compounds formed by the decomposition of animal or vegetable matter

† Cf Brieger, Ueber Ptomaine, 1885–86

Experiments on the Efficiency of Water-traps.

The means commonly employed for preventing the escape of sewer air into houses is the ordinary water-trap. Since the experiments of Nageli ('Die Niederen Pilze,' p. 109) it has been known that these traps, when acting properly, absolutely prevent the passage of micro-organisms. But it is evident that they cannot altogether prevent the passage of volatile constituents of sewer air, and we thought it worth while to make a few experiments on this point. We were not aware that the matter had already been experimentally investigated by Fergus ('The Sewage Question,' 1874), who employed methods similar to those used by us. As, however, the test substances used by us were nearly all different from those employed by Fergus, it may be well to give the results of our experiments. A leaden U-shaped trap, A, B, C, 2¾ inches in diameter, and with a seal, *a b*, of 3 inches in depth, was closed at each end, A and C, with a sheet of india-rubber stretched tightly over the mouth and fixed with wire, each sheet being perforated by a hole in the middle. The trap was then filled with water, and a glass stopper placed in the aperture E, while the neck of a flask D, containing the substance under investigation, was fixed through the india-rubber sheet at A. The whole was then left at rest, and observations made from time to time by removing the stopper and ascertaining whether the smell of the substance in D could be detected at E. In other cases a tightly fitting inverted test-tube, containing litmus or other test-paper, was inserted at E, in place of the glass stopper, and observations made as to when the test-paper was first distinctly affected. The results obtained are given in the following table:—

Substance in Flask D	Test used for detection	Depth of seal	Vol of water in trap	Time required by vapour to pass from D to E	Remarks
Oil of mustard	Smell	3 inches	890 c c	5¼—5½ hours	In leaden trap
Oil of garlic	,,	3	930	5¼—8 hours	Ditto
Oil of mint		3	910	5¼ 5½ hours	Ditto
Calcium phosphide	Silver nitrate paper and smell	3 ,,	820	5¼—7½ hours	Ditto
Putrid meat juice	Smell	3	960	About 4 days	Water very milky after the experiment and gave only a slight reaction for lead
Ammonium sulphide	Lead acetate paper and smell	3	950	Could not be detected even after 10 days	In leaden trap H_2S probably taken up by lead or oxidised
Ditto	Ditto	½ inch	90	7½—33 hours	In earthenware trap
Ammonium carbonate	Red litmus paper	3 inches	930	50—55 hours	In leaden trap
Hydrochloric acid (strong)	Blue litmus paper	3	770	Could not be detected even after 10 days	In leaden trap

Fergus found that free ammonia came through a somewhat similar trap in 15 minutes carbonic acid in 1½ hour, sulphuretted hydrogen in 3 to 4 hours, &c He also refers to similar experiments in which a ventilating pipe was placed between the substance experimented on and the trap in which the result was much the same, except that the time occupied in penetrating the trap was longer

Though it is thus the case that water traps after some time allow a certain amount of various volatile substances to pass through, yet it is hardly conceivable that the small amount thus allowed to pass can have any appreciable influence on health

We do not propose to enter here on any general discussion of the effects of the inhalation of sewer air on health The results of the foregoing investigations are clearly such as to make us much more suspicious as to supposed evidence of the bad effects of ordinary sewer air such as that of the sewers examined by us At any rate it is evident that sewer gas unless it has been vitiated by splashing, has a much less deadly composition than is often supposed It must be remembered however that the matter cannot in the present state of our knowledge be settled by analyses alone though analyses may serve as a guide in the investigation

OBITUARY NOTICES OF FELLOWS DECEASED

John Theophilus Boileau son of Thomas Boileau, at one time a well known solicitor in Calcutta, and afterwards Chief Magistrate of that city, was born there May 26th 1805 His maternal grandfather, Colonel Jessup, was an American Loyalist who suffered severely for the support he gave to the King's cause The Boileau family are of Huguenot descent

In 1819, when still much under fifteen, he was nominated a cadet to Addiscombe by Charles Marjoribanks of the E I Direction Among his contemporaries were several whose names are more or less prominent in modern Indian history, such as Henry Lawrence and his elder brother George, James Abbott (of Khiva), with his brother Sir Frederick Boileau, aged 15½, passed out of Addiscombe for the Engineers, December 19, 1820, and after a short practical training on the Trigonometrical Survey, and at Chatham under Colonel Charles Pasley, went to India, arriving at Fort William September 22nd, 1822

The Corps of Bengal Engineers was in those days a very small one, its cadre including only thirty six officers, and, if they entered it with a somewhat imperfect training, the manifold and incessant work into which the young officers were speedily plunged afforded them at least a very varied experience During the first twelve years of Boileau's service we find him engaged as executive engineer in the construction of fortifications, roads, barracks, an important church and gaol, a considerable suspension bridge, and what not, and (among other duties when stationed at Agra) on measures for the conservation of the splendid buildings left there by the Mogul dynasty, including the Taj itself

He married in 1829, and made a voyage to Europe with his family in 1837 Whilst at home he published a work which has had extensive use among surveyors, and of which he had already issued a lithographed edition in India (1836) The London publication is entitled "A new and complete Set of Traverse Tables, showing the Differences of Latitude and the Departures to Every Minute of the Quadrant, and to Five Places of Decimals, together with a Table of the Lengths of each Degree of Latitude and corresponding Degree of Longitude from the Equator to the Poles, with other Tables useful to the Surveyor and Civil Engineer By Captain J T Boileau, H E I C Bengal Engineers London W H Allen & Co, Leadenhall Street, 1839"[*]

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[*] A note in General Boileau's handwriting dated 13th November, 1880, says: 'These Traverse Tables were prepared to facilitate the computation of the areas of

It also exemplifies Boileau's constant activity of mind, that he published, at this early date, a report of his own "On the Practicability and Expense of a Plan proposed for Constructing Docks in Diamond Harbour on the River Hooghly, and for uniting them with Calcutta by a Railroad, together with an Estimate for the same."

It was at this time that the interest of the Royal Society in magnetic observation, which had been originally stimulated in 1836 by a letter of Humboldt's to the then President (the Duke of Sussex), and had been maintained by the zeal of Major Sabine, was at its height. The Society recommended the Court of Directors of the E.I. Company to take part in the institution of magnetic and meteorological observations, which (chiefly through the influence of Colonel Sykes) they decided to do. Boileau and two officers of the Madras Engineers, Lieutenants Ludlow and Elliott, were appointed to establish and take charge of observatories at Simla, Madras, and Singapore respectively, and all three went to Dublin in November, 1839, to receive from the late Rev. Dr. Humphrey Lloyd, of Trinity College, a course of that preparation for their duties which that eminent philosopher alone could then impart. Captain Boileau, before he left again for India, was elected a Fellow of the Royal Society, and also a Fellow of the Royal Astronomical Society.

The three Indian Engineer officers embarked in February 1840 for Madras, reaching that port in June, where they separated, and Boileau with his instruments proceeded to Calcutta. He reached Simla on the 24th of September, some weeks before the arrival of his assistants with the instruments. He had taken observations of dip on his palankin journey, by the way, at Allahabad, Futtehgurh, Bulandshahr, Karnál, Ambála, and Bár (at the foot of the hills on the road to Simla). He was also fortunate in securing for his magnetical campaign the hearty interest of the Rev. John Henry Pratt, afterwards Archdeacon of Calcutta, a man not less well known for his scientific acquirements than beloved for his character.

Captain Boileau selected for his observatory a site on what was then called Bentinck Hill, but which has since been known as Observatory Hill, and which in the rapid revolution of administrative events in

village lands by the officers of the Government Revenue Survey, and have gone through several editions. They are the first ever published for angular values to single minutes of arc, or to five places of decimals for distances. Their great utility, both for the above purposes and for surveying in general, has been acknowledged by letters from the United States of America, from the Brazils, from Australia, and from India." Boileau's tables are in habitual use at Cooper's Hill College. Traverse tables are intended to save the calculations of triangles in ordinary surveys, by showing by inspection the amount in linear measurement of the difference of latitude and departure (*i.e.*, of longitude) for any bearing and distance.

India has recently been selected as the site for the Viceroy's official residence.

The observations were continued from 1841 to 1846, though their maintenance had been occasionally menaced with interruption. This is referred to more than once by Boileau in the correspondence which he maintained, during the earlier years of his work at Simla, with Dr. Humphrey Lloyd. Under date 15th September, 1842, he writes:—

"We have now in Simla *all* our Chiefs, viz., Lord Ellenborough, the Governor of the N.W. Provinces, and the Commander-in-Chief. The Governor-General has not yet been to see my works, but he has expressed himself in such terms respecting the Observatory that if it is left to his Lordship's pleasure, the continuance of its observations will be short indeed. He calls the establishment of the Company's corresponding stations an English (or rather a Home) job; and I have not the least doubt that both himself and the Government of India at Calcutta are doing all they can with the Court of Directors to procure the abolition of the Indian Observatories. . . . . . Government have twice applied to me to know how long my "experiments" and "the Magnetic Survey" are to continue, and I have both times replied that the series of corresponding observations upon which I was engaged could not terminate until the 30th June, 1844, *at midnight*."

18th December, 1842: "We have got rid of all our great people, but my Lord Ellenborough has bid me attend his grand doings at Ferozpore on Christmas Day, whither accordingly I am bound on the 21st. This will cause a delay of three days in our opening the New Year, but there is no help for it. The return of our armies from Cabul promises a lasting peace to India, and I hope will also extend, almost indefinitely, the existence of the Observatory."

17th October, 1842: "I see by the paper that H.M.'s Observatories are to be continued for three years, and if any good is to come out of our work, ours must be so too, though the Governor-General told me a few days ago that I must not reckon upon more than one year more at Simla. Since then the news of our victories and the re-establishment of British supremacy in Afghanistan has come in, and a few days since the accounts of peace with China, so that the *mollia tempora fandi* have arrived, and if you desire our co-operation for a farther period, this is the time to ask for it. The peace saves our Government at least one and a half millions a year. The extra expense of four (three?) observatories is about £3,000 sterling per annum only. Lord Auckland absurdly estimated it at £10,000 a year, which was enough to frighten the Court of Directors out of their senses."

The same correspondence shows that Major Boileau not only kept the regular term-days, but certain others also privately arranged by Sir James Ross. Disturbances proved to be of pretty frequent occurrence. He sent home traces of eight in 1841, of three in the month of February, 1842, and others in August and September, 1842. These and the whole of the six years' magnetical observations remain unpublished—a circumstance which might have lent greater force to the objections of Lord Ellenborough had he been able to foresee it. There are some circumstances not easy to explain in connexion with this fact. That the magnetic observations were not *published* and *do not exist* in print is indubitable. But whether they were not *printed* is subject to a curious doubt. The Government did sanction for this purpose a printing establishment on a liberal scale, which was set up by Major Boilean first at Simla, then at Ambála (in or about 1847), and subsequently at Meerut, when he had been transferred to that station. And in a memorandum of his employments and services, in his own handwriting (dated 21st March, 1867) he enters:—"*Superintended the printing of the whole of the observations taken at the Simla station.* . . . This press was exclusively employed by the Government N.W. Provinces in printing Government work, and acquired a high reputation for the accuracy and neatness with which especially its tablework (forms of figures) was executed. *After completing the object for which it was established in connexion with the Simla Magnetic Observatory*, it was transferred to the College for Civil Engineering at Roorkee, to which it is still attached. . . ."

Also in a letter to Sir Henry Lefroy, dated 18th July, 1883, General Boileau writes:—"The whole of the records and instruments of the Simla Observatory were destroyed by fire in the year 1858, owing to the cases in which they had been packed for transmission to England, on my retirement from the Service in the beginning of 1857, having been transferred during the Mutiny from the safe depository in which they had been placed by me, into a store-room in which large quantities of *dooly* bedding had been stored away, and which had taken fire, or been set on fire, to the utter annihilation of the instruments, records, *and printed observations of the Simla Station.*"

The letter last quoted proceeds:—"Copies of the monthly abstracts, however, of the magnetic and meteorological observations of the Simla Station had been regularly forwarded by me to the Royal Society, and from them, with the sanction of the Government of India, and by the kind aid of the Royal Society, the meteorological observations of the Simla Observatory were printed and published under my superintendence in the year 1872.

"*None of the magnetical observations of the Indian Observatories have been printed;* although even at this distant time the results, if published in a condensed form, would be of great interest."

It would appear that the meteorological observations, till printed in London in 1872, stood on the same platform with the magnetical. And in the absence of any exact information, perhaps the impossibility of now recovering it, what I should deduce from Boileau's statements is this:—that all the Simla observations *were* printed by him at his Observatory press, but that for some reason, very possibly his desire to accompany the publication with prolegomena and some indication of results, for which he never found time in India, he was induced to defer their issue: that he had intended to carry the work to completion in England after his retirement and establishment in a home there; but that, in consequence of the breaking out of the Mutiny shortly after his departure from India, the despatch was delayed, and in the following year the fire occurred which consumed the whole.

We may here insert a list of books of tables of divers useful kinds which were prepared and issued by Boileau during his residence at the Observatory, or in the immediately succeeding years.

1. Tables (from Apjohn's formula) for determining the Elastic Force of Aqueous Vapour.

2. Ivory's Tables of Mean Astronomical Refractions; Revised and Augmented.

3. Mathematical Tables; comprehending Logarithms of all Numbers from 1 to 10,000, also Logarithms, Sines, Tangents, and Secants, to Six Places of Decimals.

4. Oltmann's Barometrical Tables.

5. A Collection of Tables—Astronomical, Meteorological, and Magnetical; also for Determining the Altitudes of Mountains; Comparison of French and English Weights and Measures, &c.

6. Tables of Wages and Rent; of the Value of Goods; for converting Seers and Chittacks into Decimals of a Maund, also Annas and Pice into Decimals of a Rupee.

7. Tables for facilitating the Computation of the Time from single Altitudes. Roorkee, 1858.

To these we may add:—

8. Meteorological Observations made at the Magnetic and Meteorological Observatory at Simla during the years 1841–45, under the direction of Lieut.-Colonel J. T. Boileau, F.R.S., Superintendent of the Observatory. Published by Order of the Right Hon. the Secretary of State for India in Council.

Boileau's work while at Simla was by no means confined to that of the Observatory. During the years he spent there a great variety of occasional and useful tasks were either committed to him by Government, or voluntarily undertaken.

After the Observatory work came to an end, Boileau filled the office of Superintending Engineer successively at Ambála and Meerut, till in 1854 he was transferred to Agra, on the reorganisation of the P.W. Department, as Chief Engineer to the Government of the N.W. Provinces In addition to the duties of the former office, while at Ambála he undertook to codify and co-ordinate the chaotic mass of Standing Orders of the Department, extending over a period of nearly seventy years. The result of this voluntary labour, Boileau's Code as it was called, to which he added a full Index and Series of Forms, was printed at the expense of Government, and became some years later a most valuable aid when a Committee was appointed by Government to draw up a systematic code of rules and procedure for the Department.

Colonel Boileau retired from the Service February 24th, 1857, with the usual honorary step which made him Major-General.

Boileau's life in India had been characterised by acuteness and vivacity of intellect, by an unresting and devouring activity of mind, and by extraordinary versatility and variety of work. He hardly attained the success that some of these qualities would have led one to expect, but at the same time the list perhaps suggests some reasons for this. And it is a fact that his joyous and buoyant temperament, indeed his exuberant spirits, often showing themselves in proceedings of an eccentric character, made him better known to the Anglo-Indian community than his intellectual gifts or practical accomplishments. Indeed, his sayings and doings were the subject of many widely current anecdotes, which, in some cases, were founded on the merest iota of fact, and in some others were purely mythical.

It was really in the thirty years of life following his retirement that Boileau's best qualities were drawn out to most valuable purpose, and won him wide and warm regard. He was, to be sure, as versatile and active as ever; thus he became the most energetic of vestrymen at Kensington, and as Chairman of the Building Committee which erected the handsome Town Hall there, he was indefatigable in his supervision of the work and of its financial details. He was for years a most zealous member of the Volunteer body, in which, however, he steadfastly declined the command of his corps (the 1st Middlesex Rifles) which was pressed on him, insisting on carrying his rifle as a private in the ranks. He was for some time on the Council of the Royal Society, and acted as auditor of its accounts; besides serving actively at one time or another on the Committees of various charitable, religious, or other useful Societies. But that which especially developed in General Boileau, and characterised him for the last twenty years of his life, was the active practical benevolence and devotion to the work he took upon himself as Committeeman, and eventual Chairman, of two noble institutions, viz., that of the Soldiers' Daughters' Home at Hampstead, and that of the Officers' Daughters' School at Bath (the latter having also for some years a succursal at Roehampton). To these institutions he grudged no labour, and spared no fatigue. The children of the Soldiers' Daughters' Home were always termed by him his "red chickens," and when wearied with work his greatest refreshment was to visit the Home and to get surrounded by their smiling faces and happy voices. "At any hour of the day or night, and in any weather, he would go to the Home, if his presence was required; and, as long as his strength permitted, he would sometimes walk the whole distance from his residence at Notting Hill before breakfast. On occasions of joy or sorrow at the Home he was never absent, and he was ever at the beck and call of the excellent matron, with whom he worked in unbroken harmony for the twenty years he occupied the chair. . . . All his own servants were drawn from the Home, and he would always declare that they were unsurpassed." He cared for them as if they had been his own children, and he was repaid by their attachment to him. Every girl brought up in the Home who was in London at the time of his death sent a wreath to be laid upon his coffin. Beyond his constant weekday visits to Hampstead, he for many years before his last illness maintained a practice of going there on Sunday afternoon to be present at the Bible classes which the children attended.

His exertions on behalf of the Royal School for Officers' Daughters were not less devoted. He joined the Committee of this Institution in 1872, and in 1880 became Chairman in succession to Sir Henry Lefroy, when the latter went as Governor to Tasmania. When it was deemed expedient some years ago to close the succursal at Roehampton, and to extend the buildings at Bath to receive the additional pupils there, all the details of this change and the new construction at Bath were conducted under General Boileau's close supervision.

Till his last illness he never altogether lost his buoyant spirits or his oddities; but in the constant exercise of benevolent effort these latter had taken a riper and sweeter form than in the old Indian days. In May 1886 the illness began which, with sundry fluctuations, and borne with patience and devoutness for six weary months, terminated in his death, Sunday, November 7th.

I have given some examples of his ever-active mind and versatile capacities. I may add that till he left India he played both the flute and the violin. He could, I am told, quote Hafiz with faultless pronunciation and expression; he spoke Hindustani, I know, with a vernacular swing which was rarely equalled in the mouth of an Englishman; whilst his memory was stored with old Hindu saws and rhymes, ever ready to be produced on appropriate occasions to appreciative hearers. He was also a fair Latin scholar. And for a long time he was a diligent attendant at and participator in the proceedings of the Royal United Service Institution. During the earlier discussions on rifle construction he took a serious part in them, and himself invented a rifle.

H. Y.

Sir Walter Elliot, K.C.S.I., LL.D., who died at his seat, Wolfelee, near Hawick, N.B., on the 1st March, 1887, at the mature age of 84 years, was born in Edinburgh, January 16th, 1803, the eldest son of James Elliot, Esq., of Wolfelee, by his marriage with Caroline, daughter and co-heiress of Walter Hunter, of Polmond, county Peebles. Sir Walter was educated at Haileybury College, where he obtained the certificate of "highly distinguished," and entered the service of the East India Company in 1820. In 1823 he received his first appointment as Assistant Political Agent for the South Mahratta District. After holding various other offices in the Revenue and Political Departments of the Madras Government, he was made, in 1837, Private Secretary to his cousin, Lord Elphinstone, then Governor of Madras. From 1837 to 1854 he was a Member of the Board of Revenue, and during that time was intrusted with the supervision of the Northern Circars, then in a very unsatisfactory condition. In 1854 he became Member of Council, and retained this position until he retired from the Indian Service in 1859.

Throughout his career in India and during the whole period of his subsequent life in this country until a very recent date, when his eyesight failed him, Sir Walter Elliot was constantly at work on various points connected with the Natural History, Ethnology, Antiquities, and Languages of India, in all of which subjects he was deeply versed and took the most profound interest. Though his publications were not very numerous, his notes and collections in all these departments were extensive, and were in many cases utilised in the way of contributions to the writings of his fellow-workers in these various branches of science. One of his most important earlier papers was a Catalogue of the Mammals found in the Southern Mahratta country, published in the 'Madras Journal of Science' for 1839 which was one of the first attempts made to give a connected account of the mammal-fauna of the Indian Peninsula. In the same journal and in the 'Journal of the Asiatic Society of Bengal,' will be found other zoological contributions from his pen. Sir Walter was also well acquainted with Indian plants, and after his return to this country contributed several articles to the 'Edinburgh New Philosophical Journal' on the farinaceous grains and the various kinds of pulse used in Southern India. But it is, perhaps, as an Indian antiquarian that his name will be ultimately best known to posterity. The sculptured slabs from the famous Buddhist Tope of Amrávati which adorn the walls of the great staircase in the British Museum, were procured by him, and presented to the Court of Directors, who transferred them to the national collection. Besides these, a splendid collection was accumulated at Wolfelee of coins, copper plates, arms, and other Indian ethnological objects. Sir Walter Elliot was for many years a constant attendant at the meetings of the British Association for the Advancement of Science.

Sir Walter was an ardent collector of Indian coins, and a leading authority on the subject. His principal numismatic work was a memoir on the "Coins of Southern India," which forms the second part of the third volume of the International 'Numismata Orientalia.' Sir Walter is in fact the only man who has worked systematically on Southern Indian coins, a neglected subject to which Marsden and Prinsep made some small contributions. In the work above mentioned, he has laid a solid platform, on which future Indian numismatists may proceed to build. With his habitual liberality he transferred more than 300 of his most valued coins to the collection in the British Museum. Besides this most important work, Sir Walter published two papers on the same subject in the 'Madras Journal of Literature and Science (new series, vol. 3 and 4), under the title of "Numismatic Gleanings."

In private life, it may be said in conclusion, Sir Walter Elliot was one of the kindest and most amiable of men. His sweet and genial disposition, and great liberality in every way, endeared him not only to his immediate friends and relations, but to all those with whom in various ways he came in contact. In 1839 he married Maria Dorothea, eldest daughter of Sir David Hunter Blair, Bart., of Blairquhan, who survives him, and by whom he leaves a family of three sons and two daughters.

Sir Walter was elected F.R.S. in 1878, and LL.D. of the University of Edinburgh in 1879. He was made a Knight Commander of the Star of India in 1866.

P. L. S.

SIR JOSEPH WHITWORTH was born at Stockport on December 21st, 1803. His school education terminated at the age of fourteen. He was then sent to an uncle, a cotton-spinner in Derbyshire, with the intention of his being brought up to that business. His mechanical tastes were, however, too strong, and in 1821 the idea of cotton-spinning was given up, and he obtained employment and experience for four years in the works of different machine makers in Manchester. In 1825 he went to London, and was engaged successively with several of the most important engineering firms, amongst them Maudslay and Holtzapffel. He also worked with Clement, who was engaged in the construction of Babbage's calculating machine. In 1825 he married his first wife, Fanny, youngest daughter of Mr. Richard Ankers. In 1833 Whitworth returned to Manchester, and set up as a tool maker on his own account. His business and reputation rapidly increased. In 1840 he read a paper before the British Association on his method of preparing accurate metallic plane surfaces, a method which he had devised when with Mandslay in London, and in 1841 he read a paper before the Institution of Civil Engineers on "An Uniform System of Screw Threads." During the next ten years he introduced his system of standard gauges and perfected his measuring machine, an instrument which is capable of detecting a difference in size of one millionth of an inch. In 1853 he was appointed as one of the Royal Commissioners to the New York Exhibition, and in the following year his attention was directed, at the request and with the aid of the Government, to the improvement of fire-arms. Since that time the subject of fire-arms, large or small, interested him more than any other to the day of his death. He was President of the Institution of Mechanical Engineers in 1856, and in 1857 he was elected a Fellow of this Society. In 1868 Whitworth founded the Engineering Scholarships which bear his name He provided an annual income of £3000, to be distributed as scholarships for the encouragement of the study of the theory and practice of mechanics. In July, 1869, he was created a baronet. In 1870 his first wife died, and in 1871 he married Mary Louisa, widow of Mr. Alfred Orrell. The present works of Sir Joseph Whitworth and Company, Limited, were opened in 1881. Since then they have received a rapid extension and development, and now undoubtedly contain the finest collection of powerful machine tools in the world. Of late years the state of Sir Joseph Whitworth's health usually necessitated his spending the winter in the South of France. He died on the 22nd of January, 1887, at Monte Carlo, and was buried in Darley Dale churchyard, near his country residence at Stancliffe.

One characteristic ran through the whole of Whitworth's work as an engineer—insistance upon the very highest standard of excellence both of workmanship and material, and it is to this rather than to any specific inventions or discoveries that his great success and reputation are due. The principle of his method of preparing true planes was the simple one that if any two of three surfaces accurately fit each other, each of the three must be plane; in addition to adopting this fundamental principle, he used a scraper for forming the surfaces, instead of the practice previously in vogue of grinding the surfaces together.

In the matter of screw threads and standard gauges, Whitworth insisted on the desirability of all engineers adopting the same standards, and working with them to the utmost attainable accuracy. He adopted the inch as his unit, but divided it decimally; and this introduction of the decimal system to the British workman must in itself have had a very material educational effect. In fire-arms, both small arms and artillery, the principal points on which Whitworth insisted were, the use of a long projectile with a great angular velocity of rotation about its axis, and the use of polygonal rifling of the barrel instead of grooves, the projectile being formed to fit the barrel and so secure a large bearing surface. Probably the improvements in the manufacture of steel which are associated with the name of Whitworth have done more for the development of the most modern artillery than has either of the features of his system of rifling. His improvements may be broadly said to consist in three points. first, insistance upon obtaining the best material for the purpose in the highest purity, second, in compressing the molten steel in the ingot; third, in forging under a hydraulic press instead of a steam hammer Whitworth's published writings are comparatively few in number, but these have a permanent interest and will always be instructive His fame is rather written in iron and steel, and in the daily practice of the mechanics who have been directly or indirectly trained by him, than in the journals of the learned or technical societies.

J. H.

Dr. ALLEN THOMSON, one of the most distinguished anatomists and embryologists of his time, was born in Edinburgh, Midlothian, Scotland, on the 2nd of April, 1809, and died in London at 66, Palace Gardens Terrace, on the 21st of March, 1884, in the seventy-fifth year of his age

His father, Dr. John Thomson, was a remarkable man, who at eleven years of age began life as a silk weaver's apprentice To this trade he was bound for seven years, and he continued to follow it in the town of Paisley for nearly two years after his apprenticeship had expired. His father, however, seeing that his son "took little interest in his trade," bound him in 1785 (at the age of twenty years) to Dr. White, of Paisley; and in this medical apprenticeship he continued for three years Subsequently, he became a pupil, in London, of William Hunter (brother of John Hunter), in his School of Anatomy at Leicester Square; and again returning to Edinburgh in 1793, he became a Fellow of the Royal College of Surgeons there at the age of twenty-eight. Having the year before "entered into engagements to form an alliance in business with Mr. Arrott (a Fellow of the College), he continued to live under Mr Arrott's hospitable roof till the autumn of 1798—a period of five years" In 1815 he became a Licentiate of the Royal College of Physicians of Edinburgh, and in 1808 he obtained from the University of King's College, Aberdeen, the degree of Doctor in Medicine. Having first practised in Edinburgh as a surgeon, he eventually rose to extensive practice as a physician. He was the first occupant of the Chairs of Military Surgery in the University of that city, and subsequently of General Pathology, both of which were founded on his recommendation.* In 1835, at the age of fifty-eight, he retired from active outdoor practice and his life ended at the age of eighty two He retired with the reputation of being in his time 'the most learned physician in Scotland To almost the last week of his life he was a hard student and not even fourscore years could quench his ardour in discoursing science morals, or politics He was one of the marked men of that resolute and public spirited class—the true Whig party of his day—which is now (1836) rapidly disappearing His peculiar usefulness arose neither from his talents his learning, his warmth of heart nor his steadiness of principle but from his enthusiasm He never knew apathy and medicine being his field, he was for *forty* years the most *exciting* of all our practitioners and of all our teachers Men especially young men of promise, were inspired by his zeal and his confidence in the triumph of truth' *

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* Sir Alexander Grant's 'Story of the University of Edinburgh,' vol. ii, p. 441.

His example had perhaps more influence than that of any other individual in exciting the emulation of others †

John Thomson was therefore a man acknowledged to be of great erudition as his works show, and he made many important contributions to the medical science and literature of his time from 1765 to 1846 He contributed valuable papers to the earlier numbers of the Edinburgh Review and continued (till his death in 1846) in habits of intimate friendship with its editor Lord Jeffrey continuing throughout his long life to be a man of great mark and influence in politics and science

Such was the father of Allen Thomson—the subject of this notice

To be the son of such a father was already to be born distinguished and it was a still greater distinction that throughout the life of Dr Allen Thomson, *the best characteristics of the father came to be repeated in the son*

His mother, Margaret Millar was the third daughter of Mr John Millar Professor of Jurisprudence in the University of Glasgow, to whom Dr John Thomson (then in the forty first year of his age) was married in 1806

It may be said therefore, that Allen Thomson inherited by his birth a family connexion with two out of the four Scotch Universities—an inheritance which at once gave him a position of influence, of much advantage to him in future years

Thus it came to pass that Allen Thomson was born nurtured, and trained up in an atmosphere of learning and science, so that from his very earliest years the teaching of the son by the father was such as to lay the groundwork of a solid and purely scientific career, especially as an investigator and teacher of Anatomy and Physiology

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* Journal of Henry Cockburn, a continuation of the Memorials of his Time 1831 to 1834, vol ii p 164

† Dr Richard Fowler of Salisbury in Biographical Notice of Dr Thomson in 1st vol of his Life of Cullen reissued by Blackwood and Sons, 1859

With such a father and such surroundings, he was sure to have the best up bringing and best direction For the education of his boyhood he was sent to the Edinburgh High School, and had amongst his school fellows John Murray—the eminent publisher—with whom he maintained a life long friendship also the present Lord Moncrief and Thomas Constable His professional education (mainly directed by his father) was begun at the Extra mural School and completed at the University of Edinburgh, and at the medical schools of Paris In August, 1830 (at the age of twenty one) he graduated as M D of the University of Edinburgh when his graduation thesis "on the development of the heart and blood vessels in vertebrate animals" was significant of the bent of his mind towards embryology and foreshadowed the honourable distinction which he subsequently achieved in that branch of biology which deals with developmental anatomy and physiology It was published in the 'Edinburgh New Philosophical Journal ('Jameson's Journal'), commencing in 1830 p 297 and continuing through three consecutive parts of that journal—a long contribution fully and beautifully illustrated by drawings mostly the work of his own facile pencil, and many of them coloured At the time of his graduation he was President of the Royal Medical Society of Edinburgh—a students' society which has contributed and continues to contribute, not a little to the fame of the Medical School of that city inasmuch as on the roll of its Presidents will be found the names of many, who in after-life became distinguished members and leaders in the profession * The year after graduation (1830) Allen Thomson became a Fellow of the Royal College of Surgeons of Edinburgh as a necessary preliminary to his being qualified as a teacher there It was his own wish and his father's great desire that he should become a teacher of anatomy, and devote himself to anatomical and physiological pursuits, for which he had displayed a decided predilection, and to which (as his thesis showed) he had already given a considerable share of attention

With this object in view and following the example of his friend Dr Sharpey, he travelled by himself on the Continent in 1831 His copious notes show that he then particularly interested himself in the preparations in Vrolik's Museum at Amsterdam, which he describes as a valuable collection in a state of excellent preservation In it he notes a very fine collection of skulls of different nations and

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* On its roll are inscribed the honoured names of Thomas Addison Richard Bright, Marshall Hall Henry Holland and others of Metropolitan fame with those of equal distinction associated with the Scottish and Irish Universities and Colleges, the men, in short who have been most prominent in the history of British medicine and discovery during the last hundred years 'Anatomical Memoirs of John Goodsir, vol i, p 76

ages, and among those of executed criminals of which several were those of murderers, "there is the skull of Renier, a celebrated murderer of the worst class, which Gall, when asked regarding the collection singled out and set aside as one clearly *not* belonging to a murderer!" He also notes 'a protuberance inside the inner canthus of the orbit in a Jew's skull, which Vrolik considered as peculiar to that race" Next came notes of the Strasburg and the Berlin Museums In this latter he made a great number of notes, especially bearing on the embryos of animals and the details of embryology generally dissections of varieties in the arrangement of the aorta and adjoining vessels transpositions of viscera and teratology—making drawings as well as notes of what he saw He was particularly interested in a human fœtus (481) at three weeks, in which the branchial arches are seen as well as the split between them into the pharynx "He was allowed to take this preparation from its bottle to place it in a watch glass to make a drawing of it", and to note its measurements The marks of the eye and ear were both easily seen and the superior maxillary fold connected with the inferior round the angles of the mouth He notes also the heart partially divided into two ventricles

On his return to Edinburgh after this short sojourn on the Continent he commenced his career as a teacher by starting as an extra academical lecturer on anatomy and physiology In this undertaking he was associated with the late Dr William Sharpey—(his senior by only seven years)—who ultimately became Professor of Physiology in University College, London, and with whom Allen Thomson maintained a life long friendship of the closest possible character * Six years ago (March 1881), when writing to his pupil, John Struthers, the distinguished Professor of Anatomy in Aberdeen, he says "Sharpey and I lectured together at No 9, Surgeons Square, from 1831 till 1836, when I left on account of my health being rather impaired He taught the Anatomy and I the Physiology but in the later years, as my father urged me very much to prepare myself for Anatomy, I took a share in the Anatomy teaching by attending in the dissecting room and giving some demonstrations" At this time a keen competition existed among the four teachers, who, in addition to the Professors within the University, divided among them the students who applied for instruction It was no light undertaking at that time to become a teacher of Anatomy in Edinburgh In 1828 a series of murders were brought to light which had been effected by two notorious criminals—Burke and Hare—for the money they would obtain for the bodies of their victims as material for the dissecting rooms, and for many years

* M'Kendrick Memoir of Dr Allen Thomson, 'Glasgow Phil Soc Proc, vol 15, 1884

afterwards the public mind continued to be excited by the recollection of tragedies unprecedented in the history of mankind, and which scarcely subsided even with the passing of Warburton's "Anatomy Act" in 1832, which made it possible to obtain and dissect the human body in a legal way. In the University of Edinburgh, the third Monro filled the Chair of Anatomy, himself a good anatomist of the old school, who looked upon the new teachers with an easy disregard. But while Sharpey and Allen Thomson's class between 1831 to 1836 had increased from twenty-two to eighty-eight, the majority of the students flocked to the brilliant but egotistical lectures of the famous Robert Knox, who in one year about this time had an extra-mural class of over 500 students. With unscrupulous virulence he brought his powers of ridicule and sarcasm to bear on all opponents, so that Allen Thomson and Sharpey came in for their full share.* About this time also a number of men who afterwards became famous were either students or extra-mural teachers, so that there was the keen contest of able intellects, and the rivalry of a noble ambition—the names of John Reid, Martin Barry, the two Goodsirs (John and Henry), Edward Forbes, W. B. Carpenter, John Hughes Bennett, are names which became distinguished in biological science; and in such an atmosphere of thought it is no wonder that Allen Thomson was encouraged to prosecute a purely scientific career.†

All are gone, none now remain, and the melancholy death of Dr. Carpenter in 1885 severed the last link which connected them with what was undoubtedly "a brilliant epoch in the history of the Edinburgh Medical School."

In 1833, Dr. Allen Thomson travelled with his father on the Continent for nearly three months, visiting the principal medical schools in Holland, Germany, Italy, and France. It is interesting to find in Dr. Allen Thomson's brief journal of his travels, now before the writer, such entries regarding museum specimens as he saw might be useful for teaching purposes. In these notes he frequently makes a special memorandum of the preparations which should be made for the use of his class when he had the chance of doing so on his return to Edinburgh. He gives an amusing account of his journey to London by sea in those days. Embarking on board the "Soho," from Newhaven, on the afternoon of Saturday, 6th July, 1833, after two days' sailing he reached Blackwall, whence he drove to London, and "put up at the Burlington Hotel, held by Atkinson Morley, in Cork Street, No. 35."

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* 'Life of Robert Knox, Anatomist,' by Henry Lonsdale, 1870, p. 262; also Memoirs of John Goodsir,' 1868, vol. i, page 129.

† M'Kendrick, *loc. cit.*

His first visits in London were to Dr and Mrs James Somerville, John Allen, and John Murray, and at 30, Old Burlington Street, while waiting for John Allen, he first made the acquaintance of Sydney Smith Medical education was then (1833) as now (1887), the serious subject of discussion, and Mr Allen was of opinion that ' superior degrees should be granted by Universities, with full preliminary education ensured by a degree of M A, and diplomas for general practitioners should be granted by chartered bodies, afterwards to be decided upon " He disapproved of legal prosecution of the unlicensed, unless they do harm, or take titles they have no right to

Mr Allen introduced his namesake, Allen Thomson, at this time to Lord and Lady Holland and he was afterwards introduced to Lord Melbourne by Lady Holland, at Holland House, with the words, " Melbourne, allow me to introduce to you the future Professor of Anatomy in the University of Glasgow " Allen Thomson had to wait, however, for fifteen years before that promotion took place During this visit to London he also spent some of his time with his half-brother, Dr William Thomson, James Simpson, and Dr Carswell, who was then lecturing at University College London

Dr Allen Thomson was then much interested in the work of Clift and of Owen, at the Hunterian Museum especially in the admirable series of preparations of comparative anatomy, and the beautiful manner in which vegetable structure is illustrated He also records the significant memorandum, " Make some preparations of this kind for myself " Breakfasting at Sir Astley Cooper's, he had " a very interesting demonstration from him of his preparations of the thymus gland and testicle," and mentions that " Sir Astley lectured with a great deal of spirit, and took the trouble to carry about 100 preparations from one room to another ' Allen Thomson admired particularly the dry preparations of the thymus gland at different ages, in which the sacculi of the body itself are injected with wax, the arteries, veins, and particularly the lymphatics being injected and painted of various colours Again he makes the memorandum, " Make some of these preparations in the fœtal calf " Sir Astley then demonstrated his preparation of the structure of the testicles, and again, the memorandum, " Dr Sharpey and I must have some similar injections of the tubes with wax, &c " He visited Guy's Hospital Museum with Hodgkin, and there he bears his testimony to the beauty and accuracy of the wax models of diseases of the skin and of healthy anatomy, made by Joseph Townes His father joined him on 15th of June, in London On the 17th they started for Rotterdam Thence by Dusseldorf and Delft to Bonn He gives a very detailed account of the contents of the Museum at Bonn He also describes the surgical " klin k, ' " conducted by questioning the students respecting patients committed to their care There were then two "kliniks" for medicine, by Nasse, an advanced one and an elementary one intended to teach students how to conduct the advanced one Meyer he also met, who had been Professor of Anatomy and Physiology at Bonn, and pupil of Kielmeyer, ' of whose views and lectures he spoke in terms of high admiration He met also Treviranus (the younger), Professor of Botany, Bischoff, Neumann and Weber the then Prosector He notes a case in the dissecting room of the whole body of a man in whom there was complete *situs inversus* of all the viscera, and the writer well remembers what interest Allen Thomson took in the dissection of a similar complete case of inversion of the viscera, which occurred in the dissecting room of the Glasgow University *

At Heidelberg he met Tiedemann and carefully noted the contents of his museum At Strasburg he met Ehrmann and made copious notes of his museum At Tubingen he met Autenrieth and his prosector, Professor Bauer There he visited 'the little dirty class room in which Haller and Cuvier studied Thence to Stuttgart and on to Freyburg in Baden, and to Zurich Here he met with Oken, then Rector of the University also Schoenlein Professor of Medicine and other distinguished teachers thence to Berne Lausanne and Geneva He next visited Aix en Savoie, or Aix les Bains, and Milan

At Milan he was shown great attention by Professor Panizza, the successor of Scarpa and at Parma, by Professor Tommasini, and by Pasquali, the Professor of Anatomy Here he found the University partly broken up, and about half the building occupied as a barrack for soldiers, in order to repress the revolutionary spirit of the students He notices in Professor Pasquali s Museum of Anatomy, that the dried muscular and arterial preparations were entirely painted, and puts the question, ' Why is this so seldom done it seems to preserve the preparation well, and to make it more clear ?" He afterwards in Glasgow, adopted this method to a great extent in all dried preparations At Bologna he notes that at present (August, 1833), the University is 'in disgrace and no lectures except the experimental ones are allowed to be given The Professors are obliged to give their lectures at their own homes, and soldiers are placed at the doors ' He notes that Comparative Anatomy is taught by Professor Alessandrini, under the name of 'Veterinary Anatomy,' the former title being considered by the Pope as of a revolutionary nature ' He saw some beautifully injected fœtal membranes, more particularly of the true allantoid of the mare, and of the endochorion and the decidua in the bitch

At Rome he notes that, "as in the rest of the Papal States, science is at present (September, 1833) much repressed by the fear of insubordination among the students, and that there is great difficulty in publishing or procuring scientific works.'

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* Described and figured in the Glasgow Medical Journal' for July, 1853.

At Naples, with Dr Vulpes, he met Dr Asalini, of Messina, and visited the Museo *Borbonico*, the collection of antiquities from Pompeii and Herculaneum. He then visited both these places and ascended Mount Vesuvius, the craters of which are minutely described in his carefully written journal. At the *Grotto del Cane* he saw the usual experiment of asphyxiating the dog. 'The animal fell into a faint without convulsions, and the pupil dilated at the same moment that the voluntary motions ceased, which took place in from two and a half to three minutes after the animal was placed in the cave.' Genoa and Montpellier were next visited. There, he notes a "capital series of sterno hyoid bones, bones of the skull &c. for anatomical demonstration, of which we ought to have some."

At Lyons he met Dr Bennett who was travelling with Lord Beverley and the Percy family, and renewed his acquaintance with M. Bouchet and M. Gensoul, who had both been in Edinburgh.

A very detailed but concise account is given of the anatomical, pathological and comparative anatomy preparations in Meckel's Museum, illustrated with some very beautiful pen and ink drawings, especially one of the heart in a case of partial inversion of the viscera, in which there was no *vena cava inferior*, but the *cava superior* was joined by the *vena azygos* before entering the auricle, the *venæ hepaticæ* going directly into the auricle through the diaphragm. There is also a specimen with the aorta on the right side. Similar detailed records are given of Vrolik's Museum at Amsterdam in 1831, and of the Berlin Museum.

Lastly, out of the experiences of these travels he formulates an extensive list of preparations 'to be made for teaching purposes.'

At Paris he met Rayer, Lerminier, Bouillaud, Roux and Dalmas, and visited a separate ward for cholera patients in La Charité, where he "saw three women who were recovering under the influence of opium, ice, and bleeding!'

It was by such Continental travel, with the one object before him of preparing himself for his duties as a teacher of anatomy and physiology, that he devoted himself with the greatest diligence and care to literary and scientific study, and to the study of languages. As travelling physician with the Duke of Bedford, he again spent a considerable time on the Continent, thereby perfecting his knowledge of German, French and Italian.

Of the men who mainly influenced the scientific life of Allen Thomson (besides his father's influence) there are three especially to be noted, namely, John Allen, Dr John Gordon, and Dr Sharpey.

While still a house surgeon in the Royal Infirmary of Edinburgh, the father of Allen Thomson became the friend of John Allen, who was associated with him in the duties of the house, and with whom, up to the time of Mr Allen's death in 1843, he maintained an uninterrupted friendship, and to the powerful influence of which over the fortunes of his life he has himself borne testimony in the dedication to Mr. Allen of the first volume of his 'Life of Cullen.'[*]

Dr John Thomson named his son Allen after his distinguished friend.

Dr John Gordon died in 1818, thirteen years before Allen Thomson began to lecture and teach Anatomy. After Dr John Allen had ceased to lecture, Dr John Gordon (having taught Anatomy and Physiology together for two years, 1808–1810) gave a course of Physiology separate from his course of Anatomy either in the winter ensuing or in the summer session, and for his character and work Allen Thomson had a great veneration. In writing, three years before he died, to Professor John Struthers, and referring to Gordon, he says: "I am especially pleased with your recognition of John Gordon's character and work, which is not only perfectly true as regards himself, but gives some indication of the influence which my father exercised upon his pupils and the School of Edinburgh. I have still all Gordon's papers, as well as John Allen's."

Dr Sharpey's influence was that which made itself felt through a life-long friendship of the closest kind, bound together, as he and Allen Thomson were, in allied anatomical and physiological pursuits. Sharpey was one of the young men in Edinburgh who owed the direction of their studies and inspiration to John Thomson, and this debt he repaid to his son Allen by an affectionate friendship. He was about twenty-nine years of age, and Dr Allen Thomson twenty-two, when they commenced to teach Anatomy and Physiology together in Edinburgh in 1831. This association subsisted during the four following years till 1836, when Dr Sharpey became Professor of Physiology in University College, London.

Dr. Allen Thomson spent the autumns of 1836 and 1837 at Rothimurchus, in the Western Highlands of Scotland, near Aviemore, with the Bedford family, and afterwards began his tour on the Continent with them.

On his return in 1839 he was appointed (at the age of thirty) Professor of Anatomy in the Marischal College and University of Aberdeen, which he resigned in 1841. Returning to Edinburgh in the autumn, he became once more a teacher of Anatomy in the

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[*] 'Biographical Notice of Dr. John Thomson,' prefixed to his 'Life of Cullen,' p 11, 1859. It is erroneously stated in a recent work, 'Life and Times of Sydney Smith,' by Mr. Stuart J. Reid, 1884, p 122, that John Allen and John Thomson were fellow apprentices to Mr Arnot, an Edinburgh surgeon. There was no such apprenticeship, and the facts are those stated in the text, at page xi.

Extra-mural School, No 1, Surgeon's Square, at the age of thirty-two, where he continued to give systematic lectures on Anatomy. At 11 A.M a lecture-room demonstration was given, and he taught in the dissecting room at one o'clock, assisted by demonstrators, the chief of whom was Dr Gunning, who had accompanied him from Aberdeen, and who has since given to the University of Edinburgh a fund for prizes in memory of the teachers of his day.

Professor John Struthers, of Aberdeen, bears the following testimony to the valued teachings of Allen Thomson —

"Allen Thomson's lectures on Anatomy were of a high order scientifically, and also in style contrasted favourably with the teaching in the other schools His favourite subject was embryology That could not come in much in the ordinary course, but in the summer of 1842 he delivered a special course of lectures on Development, in which he gave the results of his own researches, as well as those of the German observers. These lectures were illustrated by a very large number of beautiful diagrams, and were attended by many members of the medical profession of Edinburgh. His graduation thesis had been on the development of the heart and great blood-vessels in the vertebrata, showing an early direction of his mind to the subject of embryology. In that summer session he gave also a course of weekly lectures on the new Microscopic Anatomy, which followed the publication of the great work of Schwann In these lectures we heard much of Schwann, Henle, and Kolliker, the latter of whom became his intimate and life-long friend To this time the microscope had not been much used in the school The cell doctrine of Schwann had cleared up the confusion of the old general anatomy, although the revolution it was to effect in biology, in relation to the evolution as well as to the structure of organic forms, was hardly foreseen. Knox, whose forte was Comparative Anatomy and its bearings on human anatomy, was satirizing the microscope, as he did most things. Sharpey had been using it in the investigation of ciliary motion. John Goodsir, Conservator of the large and valuable Anatomical and Pathological Museum of the College of Surgeons, gave a few original lectures to the Fellows of the Colleges on Cells, and on Germinal Centres; and Allen Thomson used it in his researches on Development. But then, and for years afterwards, the student had nothing of the microscope beyond the privilege of a peep through Allen Thomson's and John Goodsir's on a Saturday. The work which Allen Thomson did during this year in Edinburgh secured the success of his subsequent career. His abilities as a teacher and observer were fully recognised by the medical profession of Edinburgh."

The principal reason of his apparently sudden return to Edinburgh may be explained by the fact that the Chair of Physiology in Edinburgh University was expected to become vacant by the transference of Dr Alison to the Chair of the Practice of Medicine and to succeed to the Chair of Physiology was the object of Allen Thomson's laudable ambition * Forthwith in the autumn of 1841 Professor Alison resigned the Chair of Institutes of Medicine or Physiology in the University of Edinburgh and in 1842 Allen Thomson was appointed his successor at the age of thirty three The contest was severe with such formidable competitors as Robert Knox, John Reid, Hughes Bennett and W B Carpenter He held this Professorship in Edinburgh for six years, and during that time he made several important contributions to the science of embryology He at the same time made the course on physiology systematic and complete, devoting himself entirely to the teaching of physiology proper His lectures were prepared with great care and a very elaborate synopsis of the day s subject was written in chalk on blackboards for the students to copy, supplemented by drawings in coloured chalk often very elaborate, and numerous wall diagrams (A goodly MS volume of such abstracts is in the writer s possession)

Allen Thomson s familiarity with what was being done in Germany and France gave breadth and thoroughness to his teaching He took great pains in making his drawings and in writing the heads of the lectures before the time of meeting, and like his friend Dr Sharpey, he lectured mainly from short notes He was systematic and methodical in everything and took great pains to perfect his teaching in every way, and every course of lectures he delivered whether to a popular or professional audience, cost him much labour from day to day On debatable points and where definite conclusions had not been arrived at, he was careful to give us the views of observers on opposite sides, but it was tantalising in the extreme when at the end we could not learn what his own views were This was all the more distracting because he was so full of knowledge so clear in his statement and so sound in his judgment But the weak part (or perhaps strong part) in Allen Thomson s mental development appeared to be so great an excess of caution in coming to a definite conclusion that he seemed always to hold his mind open to receive and digest new matter He was thus prevented from making any broad generalisation with which his name can be associated

In all his researches his mind inclined more to the anatomical than to the physiological side of biology, having more to do with the development of form than the development of function, and when the Chair of Anatomy in Glasgow University became vacant by the death of Dr James Jeffray in 1848 Allen Thomson became his successor at the age of thirty nine His introduction by Lady Holland to Lord Melbourne (in 1833) fifteen years before, and already referred to, shows that Allen Thomson was destined for that chair as a political

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* Memoir,' by Professor John Struthers, p 5

inheritance for then, as now, political connexion influences the chances of scientific appointments, and according as a Whig or Tory Government was in the ascendant, it was known that Allen Thomson or a political opponent would obtain the chair But when Dr Jeffray vacated his chair it was given to Allen Thomson with universal approval He thus returned to the teaching of anatomy as to his first love, remaining constant to its teaching in the Glasgow University with great distinction in the professorship He resigned it in 1877, when he was succeeded by its present distinguished occupant, Professor Cleland, who had been one of his demonstrators in previous years

During these previous twenty five years of teaching anatomy and physiology, Allen Thomson had the unique experience of having been a professor in three out of the four of the Scottish Universities and in all of them there is evidence that he worked with an indefatigable industry, not only in connexion with the immediate duties of the chair he held but as a frequent contributor to scientific literature Thus it came to pass that his reputation as a teacher and as a man of science, steadily increased, and at the end of his days he had become generally known throughout the scientific world as one of the most careful, judicious, accurate, and learned investigators and teachers of his favourite subjects His very earliest work brought him reputation as an embryologist, and herein lay his speciality, so that throughout his long and busy life he was constantly making important contributions to that department of science

He retired from his Chair of Anatomy in the University of Glasgow at the age of sixty eight having filled it for the long period of twenty-nine years and the work he accomplished there 'may be said to have been of two kinds one, the introduction of the modern anatomy and methods of teaching it, by which he laid the foundation of the eminence and success which the Glasgow School of Medicine has since attained, an object which he had warmly at heart, the other, also contributory to that end namely the planning and erection of the New University buildings, in which great undertaking he was from the beginning the moving spirit '

Succeeding a teacher who had held the Chair of Anatomy in Glasgow for the long term of fifty eight years, "it may be readily believed that Allen Thomson s anatomy and methods were a new revelation in the old monastic building of that university' As in Edinburgh, when the third Monro at last (in 1846) made way for John Goodsir, the tide turned from the extra mural school to the university, so the Glasgow School of Medicine, when Allen Thomson became Professor of Anatomy, began to take the high rank to which the new colleagues who gradually gathered round him have contributed their part *

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* 'Memoir, by Professor John Struthers, p 8

Shortly after coming to Glasgow his son and only child was born in 1849 After that he took Greenhall in 1852 about eight miles from Glasgow and afterwards Millheugh, where as he always had a great love of country life, he betook himself in the autumnal holidays, hospitably entertaining his many guests Dr Sharpey paid him regular summer visits In 1855 came the meeting of the British Association in Glasgow and among the guests then with him were his friends Professor Kolliker of Wurzburg and Professors Retzius and Broberg from Sweden In 1857 he rented Hatton House near Ratho, some miles from Edinburgh and took great interest in this old place There he became acquainted with the Lauderdale family to whom the property originally belonged and there he received his old Edinburgh friends, Syme, Bennett, Christison Douglas Maclagan, Andrew Wood, Sharpey, and Kolliker He took much interest in the garden and in garden work, and his acquaintance with Mr Archer the artist, brought him into the pursuit of photography, which gave him a new pleasure

In 1862 he left Hatton, having purchased some acres of ground at Skelmorlie on the Clyde, where he built a house in accordance with his own plans The later editions of Quain's Anatomy were written here with Dr Sharpey during the summer months and here he also enjoyed his leisure moments with his friend Professor Kolliker in the examination of marine animals

About this time the work commenced with the New University of Glasgow buildings, and from 1863, when the old college buildings were at last disposed of to a railway company, until 1870, when the classes met for the first time in the new buildings on Gilmorehill, Allen Thomson, as Chairman of Buildings Committee, was largely occupied with the anxieties and plans of the undertaking He did all the duty of a "Master of Works" and his frequent exposure during the erection of the building brought on a tendency to rheumatism with a severe attack of sciatica He further took a similarly active part in the planning and erection of the new—the Western Infirmary of Glasgow, the funds for which were mainly contributed by public subscription This hospital is generally considered to be a model hospital It has recently been greatly enlarged on the original plan, and when Dr Thomson retired from the Chair of Anatomy in 1877, he had already realised the pleasure of seeing the great success of the Glasgow Medical School which he had done so much to develop

In 1870 he ceased to live at Skelmorlie, which he let and afterwards sold, and in 1871 his only son, John Millar Thomson, settled in London as one of the assistant demonstrators on chemistry at King's College The summer of this year was spent with his son and Mrs Thomson in the Highlands of Scotland "It was," he writes in

September, 1871, "a great success for me and Mrs Allen to come here (Lynwilg, Aviemore) for a holiday. I knew the country well from my residence at Rothiemurchus (close by) with the Bedford family in the autumn of 1836 and 1837, and I had most agreeable recollections of rambles among the Grampians and various parts of the neighbourhood. Mrs Allen and I have not exactly rambled over the top of Cairngorm and Ben Muichdhui as we used to do when somewhat younger, but we have really done an immense deal of walking for such old people, and have profited in health, and enjoyed ourselves to the full. The scenery is just what we like—grand and open and yet beautifully combined with river, lake, natural wood, rock, and mountain. The birches especially are charming, and the remains of some of the old Caledonian forests of Scotch firs, some of the best existing. I am sorry to think the time of our remaining here approaches its termination, as I must be in Glasgow for meetings on the 20th, and intend to leave this on the 18th."

In 1872 he again went abroad with his family and with Dr Sharpey to visit Professor Kolliker and other friends in Germany, and in the summer of that year he extended his tour to the north of Italy. The summer of 1873 found him once more travelling in the north of Scotland, and visiting his many friends.

On his son's marriage to Miss Aikin in 1875 he began to think seriously of retiring from his professorship, and after some delay, finally ceased his active connexion with the University of Glasgow in 1877—a connexion which had extended through different members of the family without interruption for 116 years. He then came to London, where he took a house next to that of his son. Here he took an interest in all that was going on in the world of science—occupying himself especially in the affairs and work of the Royal Society. He was now able to enjoy, and he much appreciated the quiet home life he was able to lead in peaceful retirement, 'listening to the innocent prattle of his grandchildren.' It was a new pleasure and an amusement to have them with him in his study.

It is not easy to convey to others a sufficient appreciation of Dr Allen Thomson's numerous contributions to biological science. His earlier papers were on embryology, which throughout life continued to be the favourite subject of his study and researches, working hard to keep pace with the rapid progress of the science. It was in this field that he won his laurels, and although his name may not be associated in the history of that science with any one great discovery (although he contributed many new facts), yet he will always be regarded as having done much, perhaps more than anyone else in this country, to make this difficult department of biological science familiar to British biologists. He directed his attention to it when few in this country did so, and did much by clear and accurate description to make intelligible the writings of the German embryologists No one rejoiced more at the attention given to it by a rising school of British embryologists nor mourned more deeply the sad death on the Alps of its leader the late Francis M Balfour of Cambridge He was one of the first also to bring under the notice of British physiologists the researches of Weber on the tactile sensibility of the skin and he wrote largely for the Cyclopædia of Anatomy and Physiology, edited by Todd and Bowman The articles on Circulation Generation and Ovum are from his pen and to the past and current editions of the Encyclopædia Britannica he contributed articles on kindred subjects The article ' Ovum in the latter was prepared by him and over it he worked to the end with ardent love and care but since his death another hand has been employed to write it He also wrote on physiological optics more especially on the mechanism by which the eye accommodates or focuses itself for objects at different distances and his name has long been associated with current editions of Quain s System of Human Anatomy as editor especially of the descriptive parts of the seventh and eighth editions In the seventh edition he was associated with Professors Sharpey and Cleland in the eighth with Professors Sharpey and Schafer and in the ninth and last edition with Professor Schafer and Professor Thane both of University College London Alike with pen and pencil Dr Thomson made important additions to their great work especially An Outline of the Development of the Foetus He also edited a second edition of his father s Life of Cullen To the Royal Societies of Edinburgh and London, and British and Foreign medical journals he contributed numerous special papers and articles The Royal Society s Catalogue contains the titles of about twenty papers by him

During his distinguished career Dr Allen Thomson received many scientific honours He was elected a Fellow of the Royal Society of Edinburgh in 1838 and of the Royal Society of London in 1848 After his removal to London from Glasgow in 1877, he became first a Councillor of the Royal Society, and ultimately one of the Vice Presidents He was President of the Philosophical Society, of the Medico Chirurgical Society and of the Science Lectures Association in Glasgow, in which city he was also the first President of the Local Branch of the British Medical Association For eighteen years he was a member of the General Medical Council for the Universities of Glasgow and St Andrews jointly from 1859 to 1877, where his ripe experience and calm judgment enabled him to do good service to the cause of medical education In 1871 he was President of the Biological Section of the British Association at the meeting in Edinburgh, and in his address he reviewed the progress of biological science within the period of his own recollection In 1876 the Association conferred on him its highest honour by electing him to the Presidential Chair At the meeting at Plymouth in 1877, his calm, far-seeing, and philosophical address on his favourite topic, "The Development of the Forms of Animal Life ' was a masterly history of the gradual acceptance of the doctrines connected with the name of Darwin, whose important generalisations his open and receptive mind had long before accepted In 1871 Dr Allen Thomson received from his university the degree of LL D , a degree which was also conferred upon him by the Glasgow University in 1877

In 1882 he received the degree of D C L from the University of Oxford , and latterly he was elected to at least one Syndicate of the University of Cambridge to assist in the election of professors to the Biological Chairs Whilst thus pursuing a purely and steadfast scientific career Dr Allen Thomson was well known as one of the most active and influential men in the city of Glasgow

The friendly hand that wrote the obituary notice in the 'Glasgow Herald' from which much of this memoir has been compiled has given the following characteristic word picture of Dr Allen Thomson —"He took a deep interest in almost all departments of science He was a ready listener, and always delighted to hear an account of a new investigation Eager in the pursuit of truth himself he above all things, demanded accuracy He was critical on all questions, and it required a great deal of fact and argument to lead him to a change of scientific opinion Yet his mind was open and receptive, and he did not shrink from a change of view although it went against his preconceived notions His own writings are models of clearness of statement and skilful marshalling of facts Dr Allen Thomson's mode of teaching was of the same character Method, order, precision of statement and close reasoning shone in every lecture while there was also the persuasive eloquence of a great enthusiasm which captivated the listener '

As a teacher he was equalled by few and surpassed by none of the many colleagues amongst whom he taught His education and surroundings made him a teacher, and to that work he bent all his energies Those only who have been associated with him as demonstrators of anatomy in his dissecting rooms in Edinburgh, Aberdeen, and Glasgow, can appreciate the daily labours he went through in preparing and arranging the material for his lectures and demonstrations Personally he was much beloved by his students, who are generally remarkable for devotion to a teacher who takes pains to teach as he did "In the social circle there was much gentleness and simplicity of manner along with a keen sense of humour, while his domestic life throughout was characterised by a quiet kindliness, and that chivalrous attention to little details of personal courtesy which mark the true gentleman "

"One can never forget the kindly courtesy, the simplicity of address, the indescribable charm of his manner, the warmth of his friendship" Loyalty to his friends was a typical characteristic of his affectionate nature

Inheriting the best characteristics of his father with himself as with his father "he was a discerning and attached patron of youthful and friendless merit so that there are many who owe their rise in life to him, and who bless his memory all over the world * In evidence of this we have the testimony of the Treasurer of the Royal Society, in his eloquent address on December 1st 1884, when he said, in his notice of Dr Allen Thomson's death that for acts of kindness there must be many besides myself who owe him a deep debt of gratitude † The writer of this paper fully endorses this statement, and with gratitude acknowledges the many acts of kindness he received from Allen Thomson throughout a brotherly or rather father-like friendship of more than thirty six years So also Dr George Johnson in his address as President of the Medico Chirurgical Society, on March 2nd, 1885, thus spoke of him — Dr Allen Thomson will long be held in affectionate remembrance not only for the extent and variety of his scientific attainments, but for his wisdom in council, the genuine kindly courtesy which gave an indescribable charm to his manner, and the enduring warmth of his friendship ‡

On his retirement from the University of Glasgow in 1877 his portrait, painted by the President of the Royal Scottish Academy of Arts, the late Sir Daniel Macnee, was presented by his friends and admirers to the University and it now hangs in the Hunterian Museum, to hand down to future generations the cherished features of so beloved a man A replica of this portrait was presented to Mrs Thomson

Dr Thomson has left a widow and an only son Mr John Millar Thomson, Demonstrator on Chemistry in King's College, London, and Secretary to the Chemical Society of London Up to within four months of his death Dr Thomson appeared to be in excellent health, and looked forward to the pleasure of being in Edinburgh at the approaching tercentenary In 1883 he went with his wife to Cannes, on January 3rd, and spent the cold months and spring there, and the summer was once more passed with his son daughter in law, and grandchildren in the West Highlands of Scotland with much pleasure in revisiting scenes with which he had been so familiar in earlier days Suddenly, on December 14th, from his house in London, he wrote to say that his left eye had not been quite so well of late, and that Mr John Couper and Sir William Bowman

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* Memoir of H Cockburn' p 163 vol ii 1846

† Proceedings p 430 vol 37

‡ Med Chirurg Soc Trans,' vol 68, p 7

both concurred in the opinion that an iridectomy should be performed without delay. Accordingly, on the following day, Mr. Couper did an iridectomy on the left eye, on account of glaucoma. Nothing could have been more satisfactory than his recovery from the operation. The wound healed without pain, vision was maintained, and normal tension restored. About ten to fourteen days after the operation he began to experience lancinating pain referred to the ears and base of the skull, which he attributed to the east wind and rheumatism, as there was no change in either eye to account for the pain, and no external swelling He woke in the morning to find that he had been struck blind of the right eye (which up till that moment had been perfect). Mr Couper saw him forthwith, and found the tension normal, but there was a considerable area of the field of vision blind, the limit coming close to the centre He could only distinguish large letters. Next day the main branch of the *arteria centralis* was seen to be plugged with blood-clot, the portion of vessel beyond the plug empty, and so for many weeks vision was entirely lost. From this it was feared that other arteries might be similarly plugged in adjoining parts. Dr George Johnson also saw him with Mr. Couper and Mr. C. A. Aikin, and found that the cardiac valves were sound It was thus probable that the vessels became plugged owing to degeneration of their coats at the plugged part. Gradually the plugging process extended, and about the fourth month after the illness began some local paralyses became developed, first in the left hand, thumb and forefinger, which passed away, and afterwards in the muscles of the right side of the face; the vagus also became implicated, so that hiccough was more or less constant, relieved only by chloroform. His strength was reduced by protracted suffering, and eventually breathing became obstructed. He died at 66, Palace-gardens Terrace, W., on March 21st, 1884, in the seventy-fifth year of his age; and as he wrote of his friend Sharpey, so it may be written of himself: "He had not a single enemy, and he numbered among his friends all those who ever had the advantage of being in his society."* His memory will long be cherished in the hearts of thousands of his pupils at home and abroad, alike in civil and in military life

W. A.

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* 'Proceedings,' vol 31, p xix.

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